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XVII. *On Central Forces and the Conservation of Energy.*

By WALTER R. BROWNE, *M.A., M. Inst. C.E., late Fellow of Trinity College, Cambridge**.

IT is well known that the ordinary proof of the principle known as the Conservation of Energy assumes the forces acting to be Central Forces†; but the intimate connexion existing between these two facts—the existence of Central Forces and the Conservation of Energy—has not, so far as I am aware, been thoroughly examined. I shall here attempt to show that the two necessarily imply each other; so that not only is the Conservation of Energy true if the system is a system of central forces, but the Conservation of Energy is not true if the system is any thing but a system of central forces.

For the sake of simplicity I will confine myself to the case of two particles, and suppose them so far apart, in proportion to their dimensions, that each may be treated as if concentrated at its centre of gravity. Let the particles be A and B, and consider the motion of B with reference to A as fixed. Suppose B to be moving away from A, and to be acted upon by a moving force due to the action of A. Let it move from a dis-

* Read November 11, 1882.

† This is recognized explicitly by Clausius, 'Mechanical Theory of Heat,' p. 16.

tance a to a distance $a+b$, and let F be the resolved part of the moving force in the line AB. Then the energy exerted by B during this motion in overcoming the attraction of A is represented by

$$\int_a^{a+b} \mathrm{F}\,dx.$$

Let v_1 be B's initial velocity, m its mass. Then at the end of the motion v_1 will be reduced to v, where v is given by the equation

$$\frac{m}{2}(v_1^2-v^2)=\int_a^{a+b} \mathrm{F}\,dx.$$

Let us suppose b to be such that $v=0$, so that

$$\frac{m}{2}v_1^2=\int_a^{a+b} \mathrm{F}\,dx. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Then, when B arrives at distance $\overline{a+b}$, its velocity, and therefore its kinetic energy or *vis viva*, will be reduced to zero. There is therefore a loss of energy, so far as B is concerned. But now let us suppose that B is left free to return towards A, and that it passes back again over the space b. Then, if F continues to act, A will exert during the motion an amount of energy on B, or will do an amount of work upon B, which will be represented by

$$\int_{a+b}^{a} \mathrm{F}\,dx;$$

and when B has reached the distance a, it will have gained a velocity V, given by the equation

$$\frac{m}{2}\mathrm{V}^2=\int_{a+b}^{a} \mathrm{F}\,dx. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Now if $\mathrm{V}=-v_1$, then $\mathrm{V}^2=v_1^2$: hence we shall have the two particles in the same position as at first, and the kinetic energy of B will be the same as at first. Therefore there will have been no loss or gain of energy on the whole; and the energy is then said to have been *conserved* during the motion. At the time when B's velocity is zero, the energy of the system is represented by the potential energy of A—that is, the power A has of subsequently doing the work $\int_{a+b}^{a} \mathrm{F}\,dx$ upon B

At other times during the motion, the energy of the system is partly potential energy of A, partly kinetic energy of B.

We thus see that it is essential to the Conservation of Energy that V^2 should $= v_1^2$. But by equations (1) and (2) this is equivalent to the equation

$$\int_a^{a+b} F\,dx = \int_{a+b}^{a} F\,dx; \quad . \quad . \quad . \quad . \quad . \quad (3)$$

these two expressions representing the two amounts of energy exerted, as described above.

It is therefore essential for the conservation of energy that F should be a function of a form such that equation (3) may hold. We have now to inquire what this form must be, or, in other words, within what limits F may be allowed to vary so that the equation (3) may still hold.

Now we have not supposed the constitution of A and B, or their relations to each other, to vary in any way except in regard to space and in regard to time; and we have every reason to believe that these are the only variations which take place in the ultimate molecules of matter. Hence we need only consider variations with regard to space and time.

Now if F be any function of time, then, since some time must have elapsed between the exertion of the two amounts of energy represented by the two sides of equation (3), it follows that for every value of F in the right-hand expression the time will be greater than for the corresponding value of F in the left-hand expression; and therefore the sums of the two sets of values, or the two integrals, cannot be equal. Hence F cannot be a function of time.

We have therefore only to consider variations in space. Now, if we confine our attention to one plane, we know that any variation of B's place in that plane may be represented by a change in the values of x and θ; where x is B's distance from A, and θ the angle which the axis of x makes with some fixed line in the plane*. Then it is easy to show that F must not vary with θ. For if it does, let us suppose that when B has come to rest, and before it is allowed to return, it is made to rotate about A through an angle $d\theta$, and again brought to

* We here make no assumption except that the force varies as B's position in the plane varies; which is essential to every theory on the subject.

rest. Then the circumstances of A and B are unchanged; for the kinetic energy given to B during the rotation has been taken out again in stopping it. But if B is now allowed to return towards A, then, for every value of F in the right-hand expression, the value of θ will be greater or less by $d\theta$, the amount of the change, than for the corresponding value in the left-hand expression; and therefore, as before, the two integrals cannot be equal. Similarly, if we take coordinates x, θ, ϕ, in three dimensions, it will follow that F cannot be a function of ϕ.

Hence we are left with the conclusion that F can only be a function of r; in other words, the force with which A acts upon B always tends towards A, and varies, if it varies at all, according to the distance from A only. But this is the definition of a central force.

[The proof just given, that F cannot vary with θ, appears quite general. But it is easy to show that any particular law of force which can be imagined, other than that of a central force, is inconsistent with the conservation of energy. Thus, suppose the force to vary according to B's distance from some other point in the plane than A; then that distance can always be expressed in terms of the coordinates of its extremities, and therefore in an expression involving θ, which is inadmissible. Again, suppose the force to vary according to the perpendicular distance of B from some line in the plane. Then, if B move parallel to that line the force is constant, while if it be perpendicular it varies from zero; and it is easy to see that if B moves perpendicular to that line, and if, before it is allowed to return, it is rotated till the line AB is parallel to that line, then the two integrals will not be equal. Again, suppose the force to act upon a certain line only, so that when B is off that line no force acts upon it; then, if we suppose the return journey made parallel to that line, the energy on that journey is zero.]

We have throughout taken F as the force between A and B, resolved along the line joining them. We have still to consider the possibility of there being another component always at right angles to this line. This component, if it exists, will produce a rotation of B round A, which will increase B's kinetic energy; and as there will be nothing to balance it, this

increase will go on for ever; so that the conservation of energy would not be true in this case.

I have thus proved, I believe, the proposition with which I started—namely, that the doctrine of central forces and that of the conservation of energy are mutually interdependent, so that one is not true without the other. In general, as remarked at the beginning, the existence of central forces is assumed, and the conservation of energy deduced from it. But the process may be reversed. The conservation of energy may be considered to rest, as a general law of nature, on the broad basis of observed facts, such as the conversion of heat, electricity, chemical actions, &c. into mechanical work, and the reconversion of mechanical work into these other forms of energy. There can be no doubt that the evidence of this character is of very great weight; and I am myself disposed to accept it as conclusive. But it must be pointed out that, unless the above investigation be false, it involves our accepting a mechanical definition of matter substantially to the following effect*:—"Matter consists of a collection of centres of force, acting upon each other according to laws which do not vary with time but do vary with distance."

This conception of matter is of course an old one, being that usually known by the name of Boscovitch. It has not, however, been generally accepted by writers on Mechanics; and in recent times certain special objections have been raised against it, which it seems well briefly to consider.

1. An objection, due to Professor Maxwell†, is that the conception does not comprise the idea of inertia, which is a fundamental fact with regard to matter. But when we say that a body has inertia, we simply mean that a finite force, acting upon it for a finite time, generates only a finite velocity. Hence it follows that any body we can see or feel, or know and investigate in any way, must have inertia; for suppose a body to possess no inertia, then the first time any force was applied to it, it would at once be removed to an indefinite distance, and would therefore be beyond the reach of investigation. To

* This definition has been already given in a pamphlet entitled 'The Foundations of Mechanics' (Charles Griffin and Co., 1882).

† 'Theory of Heat,' p. 85.

say that matter has inertia is therefore merely to assert the general principle that any thing our senses can deal with must be finite: and it is therefore a condition anterior to any theory of matter, not a part of such theory.

2. An objection given by Lame* is that bodies, and especially homogeneous crystals, are not, within the limits of observation, denser at the centre than they are at the surface, which on the theory of central forces they apparently should be. But on this it may be observed as follows.

It may be admitted that collections of centres of force, *at rest under their mutual actions*, would be more dense towards the centre. We know no such bodies in nature. The nearest approach to it is the case of bodies so large that their molecular motions, and also their want of homogeneity, may be neglected in comparison of their mass. But the condition of large bodies does appear to agree with the theory; *e. g.* even the *mean* specific gravity of the earth (5·6) is greater than that of all bodies, except a few metals, at the surface. The want of homogeneity can have little influence at the temperature and pressure which prevail in the interior.

Again, it is known that, in all bodies, the actual centres of force must be bound up together in molecules so closely as to form coherent wholes, which no known force can change or break up. The relations in a crystal therefore are not those among separate centres of force, but among separate molecules.

Again, these molecules, being hot, are in rapid and continuous motion.

Lastly, the laws of the forces of cohesion, whether in the interior of a molecule or between one molecule and another, are unknown.

In such circumstances, can it be held impossible that there should be laws of distribution of force such that in small bodies like crystals the difference in density at the centre and surface should be insensible? Lamé does not attempt to give any rigid proof that the uniform density of crystals (even if accurately true) is really incompatible with the theory of central forces. It is therefore merely a presumption, and a presumption which seems seriously weakened by the fore-

* *Elasticité des Corps solides*, p. 333.

going considerations; it cannot therefore be allowed to have any weight as against actual evidence.

3. An objection, due to Prof. Tait, is that we have no right to assume that force has any objective existence at all, or is any thing more than the rate of change of motion—and that in fact it cannot have an objective existence, because it can be affected with a positive or negative sign. But, with regard to the first part of this objection, a force is defined in Mechanics simply as a cause of motion; and therefore the remark is a mere denial of the general principle of causation. This is not the place to discuss the truth of that principle; but it may be observed that it is perhaps almost the only principle which may claim to have been accepted by all thinkers of all schools and in all ages. With regard to the second part of the objection, the circumstance that a force, or rather the symbol of a force, may be affected, for purposes of calculation, with a + or − sign is simply due to the fact that a force has a definite sense, or direction; and that direction is one of the properties of things to which the conception of positive and negative may properly be applied. For the same reason lines may be represented as + or −, as in algebraical geometry; but they are not therefore regarded as non-existent. Nor is direction the only fact to which the conception applies; *e. g.* in treatises on algebra it is often pointed out that capital may be taken as positive and debt as negative. Will it therefore be argued that money has no real existence?

4. In some quarters an objection appears to be felt to the theory of central forces, on the ground that it involves the conception of action at a distance, which is supposed to be "unthinkable." I am not aware that the term "unthinkable," which is a new one, has ever been defined. Until it has been, it is impossible to say whether action at a distance is unthinkable, or whether the fact of a conception being unthinkable is sufficient reason, or any reason, for holding it to be untrue.

It seems desirable, before leaving the subject, to say a few words upon a theory which has been set up as a rival to that of central forces, and in some quarters has met with considerable favour. This theory supposes that bodies can act on each other only when in absolute contact; and that all the pheno-

mena of the universe may be accounted for by the knockings together of a number of ultimate atoms, considered as very small impenetrable bodies, moving with high velocities in space.

It might be urged that before such a theory can be seriously discussed, it must be shown capable of explaining (as the theory of central forces certainly does explain) the facts and principles of Mechanics. I am not aware that this has been done. I may, however, point out that the theory is not inconsistent with the conservation of energy; that is to say, it can be reconciled with it by certain special assumptions. For the proof of that principle, as given above, does not necessarily imply that the forces acting are *continuous*. If the attraction of A be supposed to act on B by equal impulses at certain intervals of space, or distances from A, which distances remain always the same, then the proof will still hold; for B will be acted upon by exactly the same number of impulses, and at exactly the same places, on its return journey as on its outward journey, and the effects will therefore be the same. Now the "collision" theory above mentioned may be taken to represent the extremest possible case of this discontinuous action—there being then but one impulse, and that acting when A and B are in absolute contact.

Let us, however, consider the assumptions involved, if the conservation of energy is to hold in this extreme case. Imagine two "ultimate atoms," of equal mass, to meet each other with equal velocities in the same straight line. This is clearly a possible case under the theory; and the conservation of energy must therefore be consistent with it. Then the instant before the atoms meet they have no action upon each other, and the instant after, by symmetry, they must either be at rest or must have passed through one another. As the latter is contrary to the hypothesis, they must be at rest. Hence a finite mass moving with a finite velocity has been brought to rest in a space infinitely small; and therefore the impulse acting upon it must have been strictly infinite in amount. This collision therefore (and it is easily seen that the same will be true of all collisions) occasions the instantaneous development of a strictly infinite force. The atoms being brought to rest, there is no reason to be given why any thing further should happen.

But we must assume it as an axiom that a further mutual impulse is then given, sufficient (if the bodies are supposed perfectly elastic) to cause each to return on its path with a velocity exactly equal to that with which it arrived. This further impulse must also be instantaneous and infinite; for, force being the cause of motion, if the impulse were finite it would at once cause the bodies to separate through an indefinitely small space, and then, *ex hyp.*, no further action could take place, and the bodies would recede from each other with indefinitely small velocities. If, then, we make these three assumptions—(1) that there is an infinite impulse developed on the collision, which brings the atoms to rest, (2) that there is a further infinite impulse, which separates them, (3) that this further impulse, while infinite, is such as exactly to reverse the previous motion of each particle—then the conservation of energy may still be supposed to hold through the collision.

It remains to ask whether there are any advantages in the collision theory such as would warrant us in discarding the principle of continuity, and in making the somewhat violent assumptions described above. The advantages specially claimed by its advocates appear to be that it does away with the conception of action at a distance, and also with that of potential energy. The latter supposition, however, is not correct. At the instant when the two atoms are at rest their actual energy is zero, and the energy existing is entirely potential, being due to their capacity of generating a return velocity equal to that of arrival. Of the former supposition I have already spoken; and I may add that I have elsewhere* shown it to be impossible to explain certain elementary facts of physics without the hypothesis of action at a distance.

XVIII. *The Electrical Resistance of Selenium Cells.*

By SHELFORD BIDWELL, *M.A., LL.B.*†

IN June 1881 a paper was read before the Physical Society by Dr. Moser, on "the Microphonic Action of Selenium Cells." In this paper a very ingenious attempt was made to

* "On Action at a Distance," Phys. Soc. 1881; Phil. Mag. Dec. 1880.

† Read November 25, 1882.

show that the effect of light in reducing the electrical resistance of selenium might be accounted for on perfectly well understood principles, without assuming the existence in the case of this substance of some law or property *sui generis* and hitherto unobserved.

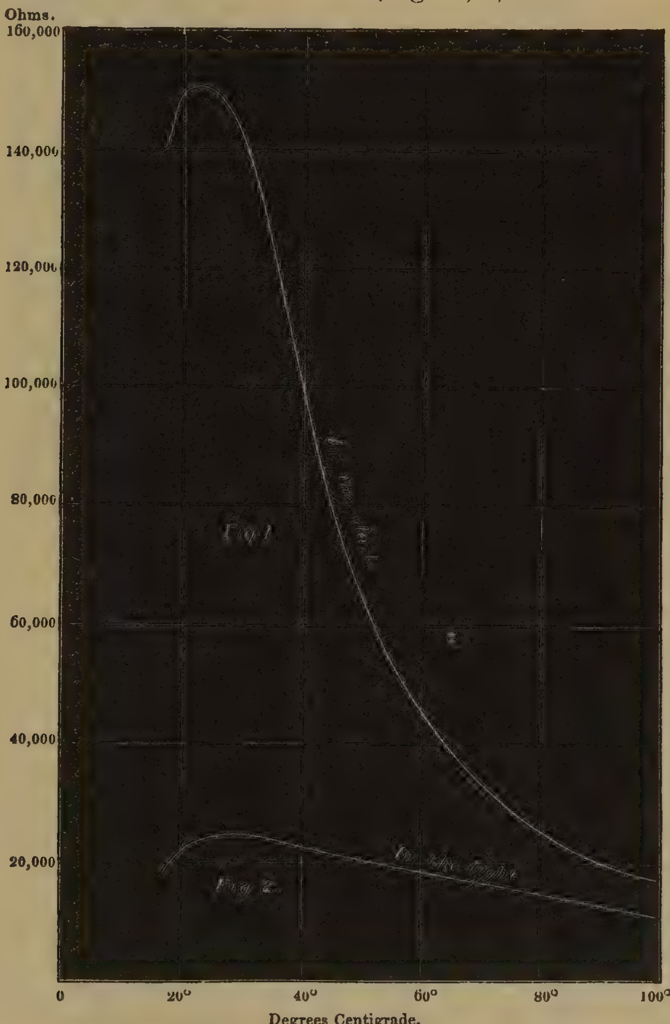
Dr. Moser's theory is shortly as follows:—There is always imperfect contact between the metallic electrodes and the selenium which together constitute a so-called "selenium cell." Selenium reflects the invisible portions of the spectrum, absorbing principally the visible or illuminating rays: the vibrations thus taken up assume the form of heat; and the temperature of the selenium cell is thereby raised*. In consequence of this rise of temperature the selenium expands; it is accordingly pressed into closer contact with the electrodes, and, as in the case of the microphone, the resistance of the system is proportionately diminished. When the cell is screened from the light, the absorbed heat is quickly radiated away; the selenium contracts to its former volume, and the original degree of resistance is restored. Thus, according to Dr. Moser's paper, the whole mystery is easily and completely explained.

This theory can evidently be submitted to a very simple and conclusive test. If it is true that the observed effects are due merely to a rise of temperature, then it is clearly immaterial whether such rise of temperature is brought about by the heating action of light or by the direct application of heat in the ordinary way. Instead of exposing a selenium cell to the light, let it be enclosed in a dark box and warmed over a gas-burner; then, if the theory be correct, the resistance of the cell should at once begin to fall. This, however, is not found to be the case. I have in my possession a number of selenium cells the resistance of which is immediately diminished by the smallest accession of light; but in the case of all of them (except one, of which I shall say more presently) the immediate effect of the direct application of heat is not a fall, but a rise in the resistance. When the temperature of the cell reaches a point which is in general a few degrees higher than the average temperature of the air a maximum

* "*Selenium*," Dr. Moser says, "*is heated by light.*"

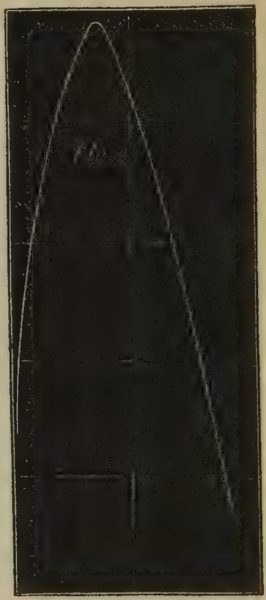
resistance is attained; and if the heating is continued, the resistance begins to decrease.

I gave a short account of this phenomenon in the 'Philosophical Magazine' of April 1881. Since this was published, I have made further and very careful experiments, the results of which are shown in the curves, figs. 1, 2, and 3.



A selenium cell was placed in an air-bath in absolute dark-

ness, the bulb of a thermometer being very near its surface. The temperature of the air was 17° C., and the resistance of the cell at the beginning of the experiment was 140,000 ohms. The bath was then very slowly heated, and the resistance measured at every degree. At first the rise was very rapid (see fig. 1); then more gradual until the temperature reached 23°, when the maximum resistance of 150,000 ohms was attained. With continued heating the resistance fell, slowly at first, then more rapidly, then again slowly (as shown by the curve), the final measurement at 100° being only 16,000 ohms*.



The same cell was afterwards submitted to the combined action of heat and light. A glass beaker was fitted with a wooden cover, to which the selenium cell was attached so as to hang perpendicularly inside the beaker; the beaker was placed in a sand-bath which was heated by a Bunsen burner, and the cell was illuminated by a powerful paraffin-lamp at a distance of 30 centimetres.

At 18° its resistance was only 19,000 ohms (see fig. 2). As in the former case, the first application of heat was accompanied by a rise, though smaller and more gradual than before, the maximum of 24,000 ohms occurring at about 29°. The fall which followed was also very gradual, the resistance at 100° being 10,000 ohms, or only 14,000 less than the maximum, as against a difference of 140,000 in the former experiment.

In these experiments it might possibly be suspected that the initial small rise of resistance is due to some accidental

* When the cell was removed from the air-bath, its resistance in the dark in air at 18° was found to have increased to 90,000 ohms.

disturbing cause, and does not point to any essential characteristic of selenium, or rather perhaps of selenium cells. The following experiment seems, however, to settle the point conclusively. One of my selenium cells (the exceptional one above referred to) did not at ordinary temperatures exhibit this peculiarity. When heated, its resistance at once went down without any preliminary rise. This cell was placed in air at a temperature of 0°, and after remaining for half an hour its resistance was found to be 147,000 ohms. The temperature was then slowly raised; and, as I expected, the resistance at first went up, attaining a maximum of 219,000 ohms at 13°, after which it went down to 134,000 ohms at 36°, when the experiment was stopped. The curve fig. 2, which is on the same scale as the others, shows the results in a very striking manner, altogether excluding the possibility of accidental disturbance. This particular cell differed from others only in the fact that it acquired its maximum resistance at a temperature slightly below instead of slightly above the average temperature of the air.

The supposition that light produces its effect by heating is further negatived by the fact, that a comparatively high degree of temperature is required to bring down the resistance of the cell to the point to which it is instantly reduced by exposure to a strong light. When a selenium cell is for a moment exposed to sunlight, it does not become perceptibly warm to the touch; but the amount of dark heat necessary to effect the same reduction in its resistance as is caused by a moment's sunshine would certainly render it too hot to handle.

Again, those who have experimented with the photophone know well that the best results are obtained only when precautions are taken to exclude those rays which are especially instrumental in producing heat, as by filtering the beam of light through a solution of alum. Dark radiation does indeed *per se* diminish the resistance of selenium; but the diminution due to dark radiation is to some extent masked by the rise of temperature which accompanies it, and which generally tends to produce the opposite effect.

To me it seems clear that the electrical effects of radiation are, in this case at least, no more due to the intermediate

action of temperature than are the chemical effects which radiation sometimes produces, as in the various photographic processes. All such effects are no doubt ultimately of a mechanical nature; but while increased temperature may result from vibrations the periodicity of which varies between very wide limits, the other effects arise only when there is some more or less definite relation between the period of the æther-waves and the molecular constitution of the substance upon which they act.

In its peculiar sensitiveness to the visible part of the spectrum selenium seems, so far as our present knowledge goes, to stand almost, if not quite, alone*.

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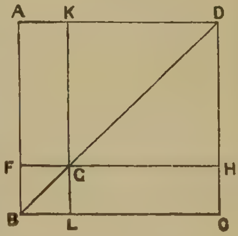
XIX. *On the Graphic Representation of the Law of Efficiency of an Electric Motor. By* Professor SILVANUS P. THOMPSON.

(1) VARIOUS graphic constructions have been given at different times to represent the work performed by an electric motor and the electric energy expended upon it. The main defect of those hitherto given has been that they present these quantities in such a manner that a comparison of the two, which would show the efficiency of working of the motor, is not immediately evident. Moreover it has not been possible hitherto to show on one construction both the law of maximum rate of working and the law of efficiency. The following construction makes them evident to the eye.

Fig. 1.



Let the vertical line A B (fig. 1) represent the electromotive force, E, of the electric supply when the motor is at rest. On A B construct a square A B C D, of which the diagonal B D may be drawn. Now measure out from the point B, along the line B A, the counter electromotive force of the motor e; this quantity will increase as the velocity of the motor increases.

* So far as regards Dr. Moser's application of his theory to the carbon

Let e attain the value B F. Let us inquire what the actual current will be, and what the energy of it; also what the work done by the motor is.

First complete the construction as follows:—Through F draw F G H parallel to B C, and through G draw K G L parallel to A B. Then the actual electromotive force at work in the machine producing a current is $E-e$, which may be represented by any of the lines A F, K G, G H, or L C. Now the electric energy expended per second is EC; and since $C=\frac{E-e}{\Sigma R}$,

$$\frac{E(E-e)}{\Sigma R};$$

and the work absorbed by the motor, *measured electrically*, is

$$\frac{e(E-e)}{\Sigma R}.$$

ΣR being a constant, the values of the two may be written respectively

$$E(E-e)$$

and

$$e(E-e).$$

Now the area of the rectangle

$$AFHD=E(E-e),$$

and that of the rectangle

$$GLCH=e(E-e).$$

The ratio of these two areas on the diagram is the efficiency of a perfect motor, under the condition of a given constant electromotive force in the electric supply.

(2) So far we have assumed that the efficiency of a motor (working with a given constant external electromotive force) is to be measured electrically. But no motor actually converts into useful mechanical effect the whole of the electric energy which it absorbs, since part of the energy is wasted in friction and part in wasteful electromagnetic reactions between the stationary and moving parts of the motor. If, however, we

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photophone of Messrs Bell and Tainter, I entirely agree with him; my own experiments showing conclusively that the effects are due to heat only. But the best carbon cells are vastly inferior in their action to those of selenium.

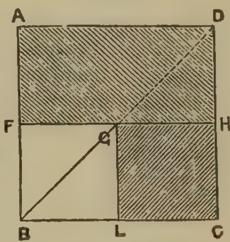
consider the motor to be a *perfect* engine (devoid of friction, not producing wasteful Foucault currents, running without sound, giving no sparks at the collecting-brushes, &c.), and capable of turning into mechanical effect 100 per cent. of the electric energy which it absorbs, then, and then only, may we take the electrical measure of the work of the motor as being a true measure of its performance. Such a "perfect" electric engine would, like the ideal "perfect" heat-engine of Carnot, be perfectly reversible. In Carnot's heat-engine it is supposed that the whole of the heat actually absorbed in the cycle of operations is converted into useful work; and in this case the efficiency is the ratio of the heat absorbed to the total heat expended. As is well known, this efficiency of the perfect heat-engine can be expressed as a function of two absolute temperatures, namely those respectively of the heater and of the refrigerator of the engine. Carnot's engine is also ideally reversible; that is to say, capable of reconverting mechanical work into heat.

The mathematical law of efficiency of a perfect electric engine illustrated in the above construction is an equally ideal case. And the efficiency can also be expressed, when the constants of the case are given, as a function of two electromotive forces. We shall return to this comparison a little later.

The Law of Maximum Rate of Working (Jacobi).

(3) Let us next consider the area G L C H of the diagram (fig. 2), which represents the work utilized in the motor. The value of this area will vary with the position of the point G, and will be a maximum when G is midway between B and D; for of all rectangles that can be inscribed in the triangle B C D, the square will have maximum area (fig. 2). But if G is midway between B and D, the rectangle G L C H will be exactly half the area of the rectangle A F H D; or, the useful work is equal to half the energy expended. When this is the case, the counter electromotive force reduces the current to half the strength it would have

Fig. 2.

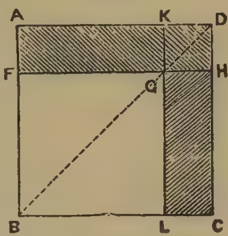


if the motor were at rest; which is Jacobi's law of the efficiency of a motor doing work at its greatest possible rate.

Law of Maximum Efficiency.

(4) Again, consider these two rectangles when the point G moves indefinitely near to D (fig. 3). We know from common geometry that the rectangle G L C H is equal to the rectangle A F G K. The area (square) K G H D, which is the excess of A F H D over A F G K, represents therefore the electric energy which is wasted in heating the resistances of the motor. That the efficiency should be a maximum the heat-waste must be a minimum. The ratio of the areas A F H D and G L C H, which represents the efficiency, can therefore only become equal to unity when the square K G H D becomes indefinitely small—that is, when the motor runs so fast that its counter electromotive force e differs from E by an indefinitely small quantity only.

Fig. 3.



Further, it is clear that if our diagram is to be drawn to represent any given efficiency (for example, an efficiency of 90 per cent.), then the point G must be taken so that area G L C H = $\frac{9}{10}$ area A F H D; or, G must be $\frac{9}{10}$ of the whole distance along from B towards D. This involves that e shall be equal to $\frac{9}{10}$ of E; which expresses geometrically the law of maximum efficiency.

It is strange that even in many of the accepted text-books this law is ignored or misunderstood. It is indeed frequent to find Jacobi's law of maximum rate of working stated as the law of efficiency. Yet as a mathematical expression the law has been known for many years. It is implicitly contained in more than one of the memoirs of Joule; it is implied also in more than one passage of the memoirs of Jacobi*; it exists

* Jacobi seems very clearly to have understood that his law was a law of maximum working, but not to have understood that it was not a law of true economical efficiency. In one passage (*Annales de Chimie et de Physique*, t. xxxiv. (1852) p. 480) he says:—“Le travail mécanique maximum, *ou plutôt l'effet économique*, n'est nullement compliqué avec ce que M. Muller appelle les circonstances spécifiques des moteurs électromag-

in the *Théorie Mécanique de la Chaleur* of Verdet*. Yet it remained a mere mathematical abstraction until its significance was pointed out three or four years ago by Siemens.

(5) Further, if the motor be not a "perfect" one, but one whose intrinsic efficiency, or *efficiency per se*, is known, the actual mechanical work performed by the motor can be represented on the diagram by simply retrenching from the rectangle G L C H the fraction of work lost in friction &c. Similarly, in the case where the electric energy expended has been generated in a dynamo-electric machine whose intrinsic efficiency is known, the total mechanical work expended can be represented by adding on to the area A F H D the proportion spent on useless friction &c. To make the diagram still more expressive, we may divide the area K G H D into slices proportional to the several resistances of the circuit; and the areas of these several slices will represent the heat wasted in the respective parts of the circuit. These points are exempli-

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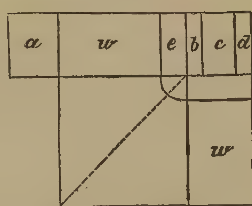
nétiques." Yet, though here there is apparently a confusion between the two very different laws, in a preceding part of the very same memoir Jacobi says (p. 466):—"En divisant la quantité de travail par la dépense (de zinc), on obtient une expression tres-importante dans la mécanique industrielle: c'est l'effet économique, ou ce que les Anglais appellent *duty*." Here, again, is a singular confusion. The definition is perfect; but "effet économique" is not the same thing as the maximum power. Jacobi's law is not a law of maximum efficiency, but a law of maximum power; and that is where the error creeps in. It is significant, in suggesting the cause of this remarkable conflict of ideas, that throughout this memoir Jacobi speaks of *work* as being the product of force and velocity, not of force and displacement. The same mistake—common enough amongst continental writers—is to be found in the accounts of Jacobi's law given in Verdet's *Théorie mécanique de la Chaleur*, in Müller's *Lehrbuch der Physik*, and even in Wiedemann's *Galvanismus*. Now the product of force and velocity is not work, but work divided by time—that is to say, rate-of-working, or "power." This may account for the widely-spread fallacy. Jacobi makes another curious slip in the memoir above alluded to (p. 463), by supposing that the strength of the current can only become =0 when the motor runs *at an infinite speed*. We all know now that the current will be reduced to zero when the counter electromotive force of the motor equals that of the external supply; and if this is finite, the velocity of the motor, if there is independent magnetism in its magnets, need also only be finite. This error—also to be found in Verdet—seems to have thrown the latter off the track of the true law of efficiency, and to have made him fall back on Jacobi's law.

* Verdet, *Œuvres*, t. ix. p. 174.

fied in fig. 4, which represents the transmission of power between two dynamos, each supposed to have an intrinsic efficiency of 80 per cent., each having 500 ohms resistance, working through a line of 1000 ohms resistance, the electromotive force of the machine used as generator being 2400 volts, and the counter electromotive force of the machine used as motor being 1600 volts.

Fig. 4.

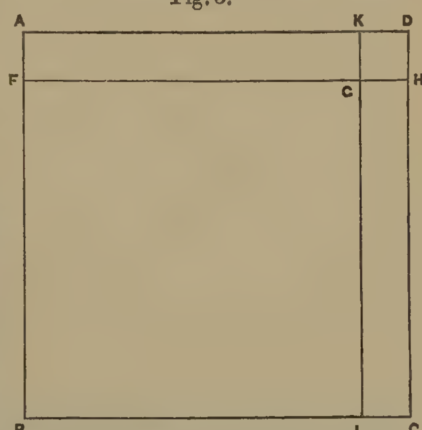


The entire upper area represents the total mechanical work expended. Call this 100, and it is expended as follows :—$a = 20$, lost by friction &c. in the generator; $b = 6\frac{2}{3}$, lost in heating generator; $c = 13\frac{1}{3}$, lost in heating line-wires; $d = 6\frac{2}{3}$, lost in heating motor; $e = 10\frac{2}{3}$, lost in friction in the motor; $w = 42\frac{2}{3}$ is the percentage realized as useful mechanical work.

(6) The advantage derived in the case of the electric transmission of power from the employment of very high electromotive forces in the two machines is also deducible from the diagram.

Let fig. 3, given above, be taken as representing the case where E is 100 volts and e 80 volts. Now suppose the resistances of the circuit to remain the same while E is increased to 200 volts and e to 180 volts. (This can be accomplished by increasing the speed of both machines to the requisite degrees.) $E - e$ is still 20 volts, and the current will be the same as before. Fig. 5 represents this state of things. The square K G H D which represents the heat-waste is the same size as befoer; but the energy spent is twice as great, and the useful work done is more than twice as great as previously. High elec-

Fig. 5.

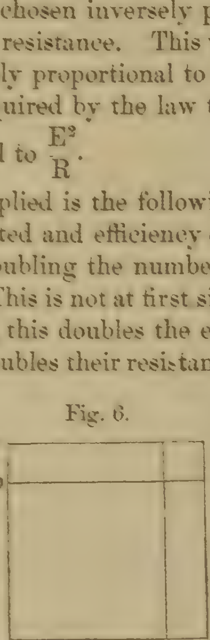


tromotive force therefore means not only a greater quantity of power transmitted, but a higher efficiency of transmission also. The efficiency of the system in the case of fig. 3 was 80 per cent.: in the case of fig. 5 it is 90 (the dynamos used being supposed "perfect"); and whilst double energy is expended, the useful return has risen in the ratio of 9 to 4.

(7) So far it has been supposed that the resistance of the system is a constant quantity. But it is possible to construct diagrams in which changes of resistance are taken into account. All that is necessary is to vary the scale of the diagram, the linear unit of scale being chosen inversely proportional to the square root of the total resistance. This will make the areas of the diagrams inversely proportional to the resistances in the different cases, as required by the law that the energy of the current is proportional to $\frac{E^2}{R}$.

An example in which this rule is applied is the following. It can be shown that the power transmitted and efficiency of a transmitting system are increased by doubling the number of coils in the armatures of the machines. This is not at first sight self-evident; for though, *cæteris paribus*, this doubles the electromotive force of the machines, it also doubles their resistances. Let fig. 6 be the diagram for a transmitting system, where $e = \frac{4}{5}E$, and in which these values are both going to be doubled by doubling the number of armature-coils. There are two cases to consider:—(*a*) first, where the line-resistance is very small compared with that of the two machines; (*b*) second, where the line-resistance is very large compared with that of the two machines.

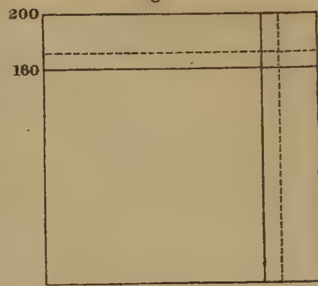
Fig. 6.



(*a*) In the former case, where we neglect the resistance of the line, we must draw a diagram diminishing the linear unit of scale to $\frac{1}{\sqrt{2}}$ of its value. But as on this scale we are going to represent doubled electromotive forces, the actual figure will have to be $\sqrt{2}$ times as large as fig. 6. Draw, then, the

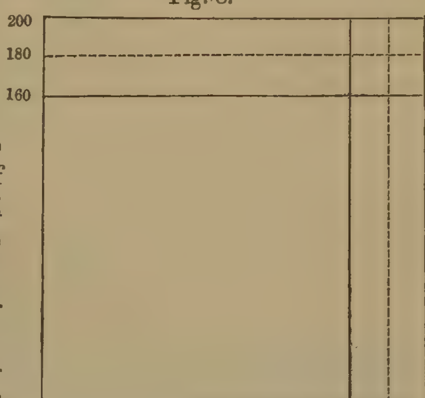
fig. 7, taking as side of the square a line equal to the diagonal of fig. 6, and we obtain a diagram in which, while the efficiency is the same as before, the actual quantity of work effected in unit time is doubled. For the areas representing respectively energy expended, work done, and heat-waste are in fig. 7 double of those in fig. 6. But no such case can occur in practice, as the line must have some resistance. Then doubling the number of coils of the machines will not cut down the scale so greatly as we have supposed; and the work transmitted will be more than doubled. Further, if the number of coils on the machine used as motor be a little more than doubled, a higher efficiency will be attained; since then the area of the square K G H D will be further diminished, while the scale on which the diagram is drawn will only be very slightly diminished. If diminished, as shown by the dotted lines, so that $e - E$ has the same value as before, the efficiency will be a little less than doubled, the power transmitted remaining as at first.

Fig. 7.



(*b*) If the case where the line-resistance is very great as compared with the resistances of the machines be taken, we find that doubling the number of the coils of the two machines will double their respective electromotive forces, without altering appreciably the total resistance or the scale of the diagram. To represent this change relatively to fig. 6, we must reconstruct that figure, doubling its linear dimensions each way, as in fig. 8. It is at once evident that the power transmitted is increased fourfold, while the efficiency remains the same. If we increase the

Fig. 8.



number of coils, as before, on the machine at the receiving end of the line so as to bring up the difference $E-e$ to the value it had in fig. 6, the scale of the diagram will still be unaltered; the power transmitted will be now only double instead of quadruple; but the efficiency will thereby be more than doubled, the heat-waste being the same, and the energy utilized more than twice as great. High electromotive force is therefore advantageous in both cases, especially in the case of a great resistance in the line.

(8) It only remains to point out a curious contrast that presents itself between the efficiency of a perfect heat-engine and that of a perfect electric engine. We saw (§ 2) that the one could be expressed as a function of two temperatures, whilst the other could be expressed as a function of two electromotive forces. But in the heat-engine the efficiency is the greatest when the difference between the two temperatures is a maximum; whilst in the electric engine the efficiency is the greatest when the difference between the two electromotive forces is a minimum. The two cases are contrasted in figs. 9 and 10, fig. 9 showing the efficiency of a heat-engine working between temperatures T and t (reckoned from absolute zero); whilst fig. 10 shows the efficiency of an electric engine receiving current at an electromotive force E, its counter electromotive force being e.

Fig. 9.

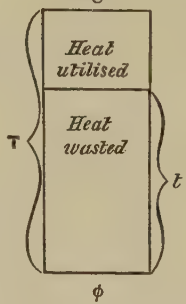
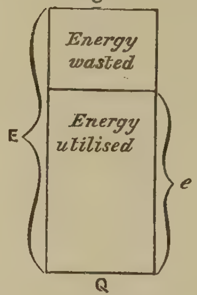


Fig. 10.



Joule's remark, here illustrated, that an electric engine may be readily made to be a far more efficient engine than any steam-engine, is amply justified by all experience. But in spite of this fact, electric engines are, as yet, dearer in practice than heat-engines, simply because energy in the form of electric currents supplied at a high potential is, as yet, much more costly to produce than energy in the form of heat supplied at a high temperature.

XX. *On the Spectra formed by Curved Diffraction-gratings.* *By* Walter Baily*.

[Plate VIII.]

In the curved diffraction-gratings invented by Professor Rowland, he has pointed out that if a source of light be placed at the centre of curvature, all the rays diffracted back from the grating will have their foci on the circle which lies in a plane perpendicular to the lines of the grating, and is described on the radius of curvature as diameter. In this paper I investigate the locus of these foci, and of those of rays transmitted through a transparent grating for any position of the source of light in the same plane.

Let a plane grating be placed at D (fig. 1) with its lines perpendicular to the paper and one of them passing through D, and its plane perpendicular to C D, and let a D be an incident ray, of which a portion with wave-length λ_1 is diffracted along Da_1, and a portion with wave-length λ_2 is diffracted along Da_2, a_1Da_2 being a straight line. Let $CDa=\theta'$, and $CDa_1=\theta$; and let σ be the distance between the lines of the grating. Let D′ be the next line of the grating to D; and draw $D'a'$ and $a'_1D'a'_2$ parallel to Da and a_1Da_2 respectively; and draw aa', $a_1a'_1$, $a_2a'_2$ perpendicular to Da and a_1Da_2. Then we must have

$$aD+Da_1=a'D'+D'a_1'+n_1\lambda_1$$

and

$$aD+Da_2=a'D'+D'a'_2+n_2\lambda_2,$$

where n_1 and n_2 are integers.

These equations give us

$$\sigma(\sin\theta'+\sin\theta)=n_1\lambda_1,$$
$$\sigma(\sin\theta'-\sin\theta)=n_2\lambda_2.$$

Now (fig. 2) let D be the centre of a cylindrical grating whose lines are perpendicular to the plane of the paper, C the centre of curvature, and $CD=c$. Let P be the source of light, Q the focus of a diffracted ray, E a point on the grating near to D. Join PD, PE, QD, QE, CE. Let $CP=a$, $CQ=b$, $\angle PCD=\alpha$, $QCD=\beta$; $DP=r$; $DQ=r'$, $\angle CDP=\theta$,

* Read January 27, 1883.

$\angle CDQ=\theta'$, $\angle DCE=\gamma$. Then we have for light diffracted back from the grating, which we may call "reflected light,"

$$\sin CEP+\sin CEQ=n\frac{\lambda}{\sigma},$$

$$\frac{a\sin(\alpha-\gamma)}{\{a^2+c^2-2ac\cos(\alpha-\gamma)\}^{\frac{1}{2}}}+\frac{b\sin(\beta-\gamma)}{\{b^2+c^2-2bc\cos(\beta-\gamma)\}^{\frac{1}{2}}}=\frac{n\lambda}{\sigma}.$$

Expanding in terms of γ, we get

$$\frac{a\sin\alpha}{\{a^2+c^2-2ac\cos\alpha\}^{\frac{1}{2}}}+\frac{b\sin\beta}{\{b^2+c^2-2bc\cos\beta\}^{\frac{1}{2}}}-\frac{n\lambda}{\sigma}$$

$$+\left[\frac{(a^2+c^2-2ac\cos\alpha)a\cos\alpha-a^2\sin^2\alpha}{(a^2+c^2-2ac\cos\alpha)^{\frac{3}{2}}}\right.$$

$$\left.+\frac{(b^2+c^2-2bc\cos\beta)\cos\beta-b^2\sin^2\beta}{(b^2+c^2-2bc\cos\beta)^{3}}\right]\gamma$$

$+$ terms involving higher powers of $\gamma=0$.

Putting

$$a\cos\alpha=c-r\cos\theta,\qquad b\cos\beta=c-r'\cos\theta',$$

$$a\sin\alpha=c\sin\theta,\qquad b\sin\beta=c\sin\theta',$$

we get

$$\sin\theta+\sin\theta'-n\frac{\lambda}{\sigma}+\left[\frac{\cos\theta}{c}-\frac{\cos^2\theta}{r}+\frac{\cos\theta'}{c}-\frac{\cos^2\theta'}{r'}\right]c\gamma+\&c.=0.$$

This equation must be satisfied for all very small values of γ. Hence

$$\sin\theta+\sin\theta'=\frac{n\lambda}{\sigma};$$

and

$$\frac{\cos^2\theta}{r}=\frac{\cos\theta}{c}+\frac{1}{d},$$

$$\frac{\cos^2\theta'}{r'}=\frac{\cos\theta'}{c}-\frac{1}{d},$$

where d is any quantity.

In the last equation put $180+\theta'$ for θ', and $-r'$ for r'. The equation then becomes identical with the corresponding equation. Hence curves whose equation is

$$\frac{\cos^2\theta}{r}=\frac{\cos\theta}{c}+\frac{1}{d}$$

have the property that, if the source of light is at any point on one of these curves, the whole of the reflected spectra produced by the grating lie on the same curve.

If we start with the equation for transmitted light,

$$\sin\theta' - \sin\theta = n\frac{\lambda}{\sigma},$$

we shall arrive at the same result. Hence we see that each of the curves whose equation has just been found is the locus of the foci of all the diffracted rays, when the source of light is at any point on the curve.

These curves I will call "diffraction-curves." It is obvious from the equation that they are independent of the distance between the lines of the grating.

A table is given at the end of the paper showing the values of r for every five degrees in the value of θ, d having the values $\frac{c}{3}$, c, $3c$, c being taken as 1000; and the forms of the curves are shown in fig. 3.

When d is infinite, the diffraction-curve is a circle having the radius of curvature of the grating as diameter, and a straight line through D tangential to the grating. In every other case the curve is formed of two loops, one lying inside the circle and the other outside, touching one another at D. The inner loop is always an oval, which is infinitely small when d is zero, and increases as d increases, until d becomes infinite, when the inner loop coincides with the diffraction-circle. The outer loop is finite when d is less than c; and increases as d increases, until d equals c, when the outer loop becomes infinite, and resembles a parabola. When d is greater than c the outer loop takes somewhat the form of an hyperbola, with the asymptotes inclined to the axis at an angle whose cosine is $\frac{c}{d}$, and intersecting one another at a distance from the grating $=\frac{c^3}{d^2-c^2}$. One of the two branches into which the outer loop is now divided passes through D, always retaining the resemblance to a branch of an hyperbola, and ultimately, when d is infinite, becomes a straight line tangential to the grating. The other branch has points of inflection, if d is greater than $\frac{3}{2}c$, in the positions for which $3c\cos\theta = d - \sqrt{18c^2+d^2}$; and when d is greater than $2c$, this branch has points which are at a minimum distance from D. At these points the dis-

tance from D is $\frac{4c^2}{d}$, and $\cos\theta = -\frac{2c}{d}$. Consequently the locus of these points is the circle of curvature of the grating. When d becomes infinite, this branch coincides with the tangent to the grating at D, and with the diffraction-circle.

The diffraction-curve has been shown to consist of two loops, one of which passes through the source of light. This loop is the locus of the spectra of transmitted light; and the wave-length at any point is given by the equation

$$n\lambda = \sigma(\sin\theta' - \sin\theta).$$

The other loop is the locus of the spectra of reflected light; and the wave-length at any point is given by the equation

$$n\lambda = \sigma(\sin\theta' + \sin\theta).$$

As both loops coincide in the diffraction-circle, this circle is the locus both of the spectra of transmitted and of reflected light when the source of light is on the circle.

As an example of the determination of the wave-length, suppose the grating to have 25,000 lines to the inch; then each division of the grating is 40 millionths of an inch. Divide the diameter of the diffraction-circle into 40 parts, and with the centre of curvature as centre describe circles through these divisions, and number the points in which they cut the diffraction-circle, beginning with the centre of curvature as zero, and counting the readings as positive on one side of the zero and negative on the other (see fig. 4, in which only every tenth reading is given). If the source of light be at the centre of curvature, the readings of the diffraction-circle will give the wave-lengths, or multiples of them, in millionths of an inch. Now with the centre of the grating as centre of projection, project the readings of the diffraction-circle on both branches of any diffraction-curve, and place the source of light at the point in which one of the loops cuts the perpendicular from the centre of the grating. The readings give the wave-lengths, or multiples of them, as before.

Let the source of light be now placed at any point of a graduated diffraction-curve. Take the reading of the point, and *subtract* it from the readings of all other points on the *same* loop; the new readings will give the wave-lengths, or mul-

tiples of them, for transmitted light. *Add* the reading of the position of the source of light to the readings of all points on the *other* loop, and the new readings will give the wave-lengths, or multiples of them, for reflected light. The diffraction-circle must be treated as two distinct loops, and the reading of the source be subtracted from the readings on the circle for transmitted, and added for reflected light. One of the zero-readings occurs at the source of light, and the other at the focus of ordinary reflected light.

In the case of a plane grating, since c is infinite, the equation to the diffraction-curve becomes

$$r = d \cos^2 \theta.$$

TABLE showing the values of r for every five degrees of θ in the equation

$$\frac{\cos^2 \theta}{r} = \frac{\cos \theta}{c} + \frac{1}{d}.$$

$\pm\theta$.	$d=\frac{c}{3}$.	$d=c$.	$d=3c$.	$d=\frac{c}{3}$.	$d=c$.	$d=3c$.	$\pm\theta$.
90	0	0	0	0	0	0	90
85	2	7	18	3	8	31	95
80	9	20	77	11	37	190	100
75	21	53	113	25	90	899	105
...	...	...	...	...	...	13691	109
70	35	87	173	44	178	−13451	110
65	52	126	231	73	309	− 2000	115
60	71	167	300	100	500	− 1500	120
55	92	209	363	140	772	− 1372	125
50	113	252	423	175	1156	− 1335	130
45	135	293	481	224	1707	− 1338	135
40	156	332	535	263	2508	− 1356	140
35	176	369	582	314	3710	− 1382	145
30	194	402	625	351	5603	− 1408	150
25	210	431	663	392	8768	− 1434	155
20	224	455	694	429	14640	− 1456	160
15	235	475	718	459	27394	− 1475	165
10	243	489	736	481	63890	− 1489	170
5	248	497	746	495	261165	− 1497	175
0	250	500	750	500	∞	− 1500	180

$c = 1000$.

XXI. *Optical Combinations of Crystalline Films.* *By* Lewis Wright.

[Plate IX.]

The object of the following experiments is to illustrate the facility with which simple combinations of mica-films, such as can be readily put together by any one with the aid of Canada balsam dissolved in benzol, may be made to demonstrate not only the simpler chromatic phenomena of polarized light, but also the more beautiful and complicated appearances encountered at a more advanced stage of study. The colours obtainable from such mica-films are more delicate and intense than the usual selenite preparations, because while in selenite those films which produce the lower and more intense orders of Newton's colours are so thin as to be split with difficulty, in mica they can be obtained with the greatest facility. Some of the preparations are also, as demonstrations, superior in themselves.

Let us take first the simplest case, of different retardations produced by different thicknesses of crystal, counteracted or not by opposite retardations caused by another crossed film. It has been usual to demonstrate these by two selenite wedges, rotating one over the other. A simpler and more effective demonstration is given by two wedges, each built up of similar mica-films superposed, and cemented together, like those on the screen (Pl. IX. fig. 1). The series of flat steps or tints are both more conspicuous and more readily understood; and if the two wedges are properly matched, when crossed the diagonal row of squares will be black when the Nicols are crossed. When the thick edge of one is superposed over the thin edge of the other, with the mica axes parallel, we have an even tint; and when the thick edge is superposed over the other thick edge, with the mica axes crossed, the retardations or colours produced by the first wedge are all destroyed by the second, and the field is all black. I wish to remark here, as I have done elsewhere, that the idea of wedges and other designs, built up in this way of thin flat films, is not due to me, but to my friend Mr. Fox, F.R.M.S., from whom both this pair of wedges and the next one are simply copied.

Here is another preparation of the same character, built up

of twenty-four films, each of a thickness causing exactly $\frac{1}{8}\lambda$ of retardation for yellow light. Of course the thickness must be very exact to bear multiplication twenty-four times without sensible error. Now if we superpose on this wedge a flat plate of mica with its axis* crossing that of the wedge, and of a thickness equal to the middle stripe, that central stripe must appear black when the Nicols are crossed (fig. 2), while Newton's first order of colours and half the second order appear symmetrically on both sides of the black stripe. On one side of the stripe the wedge itself gives greater successive thicknesses; while on the other side the plate of mica does the same.

Such a wedge has further and real optical uses. It shows at a glance the precise composition of every successively increased $\frac{1}{8}\lambda$ of retardation for the first three orders of Newton's colours. When extinction is complete for yellow light, we know that a little of both red and blue must be unextinguished, the two giving us at the end of the first order the opaque plum-colour known as the "tint of passage." As the red at a given distance from the end of the spectrum is visually more conspicuous than the blue, at the end of the second and third orders this "tint of passage" must become more and more red, as we see on the screen is the case. The precise composition of the light destroyed and that remaining, we may demonstrate by placing a slit across the wedge and throwing its spectrum on the screen (fig. 3), when we see the shifting of the bands in steps for each $\frac{1}{8}\lambda$ of retardation. The wedge alone shows only the first three orders; but it is obvious that by superposing a plate of mica 1λ in thickness, the spectrum would give us from the second to the fourth orders, and so on. I have not here the plates to show this in detail; but I have brought a thick plate of selenite, not measured, but hurriedly mounted for this afternoon. We throw its spectrum on the screen first: from the seven or eight dark bands it appears to be from eight to ten waves in thickness; it is at all events so thick as to show no colour. But now superposing the wedge, the shifting of the bands shows the precise composition for every successive $\frac{1}{8}\lambda$ of retardation even in this high

* Throughout this paper the "axis" of the mica is supposed to be that one of its two polarizing planes which passes through the two optic axes.

order of interferences. It is all rendered by spectrum analysis. Another great use of such a wedge is for gauging the thickness of films in making other preparations, for which I use it constantly: we only have to superpose the film to be gauged with its axis crossing that of the wedge, and the stripe that is nearest extinction when the Nicol is crossed gives the thickness.

The *rotatory* colours of films are also beautifully shown by mica preparations. We all know that if a film $\frac{1}{4}\lambda$ thick (the terms "thick" or "thickness" of course mean in retardation of the slowest ray, throughout this paper) is adjusted with its polarizing planes at 45° with the plane of polarization, we obtain a single circular vibration. But if we adjust in this position a film giving colour next the polarizer, and introduce after that the $\frac{1}{4}\lambda$ plate, with its planes at an angle of 45° with those of the colour-film, both the two rays which emerge from the first film are converted into rays circularly polarized, but in opposite directions; and hence we get approximately the rotatory colours of quartz as the analyzer is rotated. The geometrical figure I now insert is thus circularly polarized, and will illustrate not only the beautiful rotational phenomena of the colours, but also that superior delicacy and intensity of these lower-order colours which has been alluded to: it would be exceedingly difficult to get colours like these in selenite. Again, we take the 24-section wedge used just now, and superpose upon it a $\frac{1}{4}\lambda$ plate made in two halves, one of which has its planes reversed as compared with the other; on rotating the analyzer the colours appear to pass along the wedge in opposite directions, as if it were made in two halves of right- and left-handed quartz.

My friend the Rev. P. R. Sleeman lately suggested to me another preparation, which was in turn suggested to him by a beautiful one in quartz belonging to the President of the Royal Society. This is a quarter-wave plate divided into twelve sectors. In the position now on the screen the polarizing planes are all perpendicular and horizontal; but the principal plane or "axis" is *reversed* (as in fig. 4) in every alternate sector. If we superpose this upon a mica-film giving uniform colour, on rotating the analyzer we get, as you see, the contrary quartz rotations. But it lately occurred to me that a still more beautiful demonstration of these rotational

colours would be obtained by another combination, which deserves perhaps to be called an "optical chromatrope." We place first in the stage next the polarizer a large even-tint film in a rotating frame; next to that a concave selenite plate showing Newton's rings; next to that again our quarter-wave plate in sectors. As we rotate the analyzer, one set of alternate sectors of the rings approach the centre, while the intermediate sectors recede from it; and if we now at the same time rotate the even-tint plate, we simultaneously vary the *colour* phenomena in an exquisitely beautiful manner.

A $\frac{1}{4}\lambda$ plate divided into four sectors or quadrants, with their planes alternately reversed in the same way (fig. 5), enables us to demonstrate the nature of the curious modifications of the rings and brushes in a plate of crystal when circularly-polarized convergent light is employed. Here, for example, are the rings and cross of calcite: interposing a $\frac{1}{4}\lambda$ plate, the black cross disappears into a grey nebulous one, and on opposite sides of each arm the quadrants of rings appear dislocated, the dark rings of one quadrant opposing the light rings of its neighbours. Interposing another $\frac{1}{4}\lambda$ plate on the other side, on rotating the analyzer one opposite pair of quadrants contracts while the intermediate ones expand, so that in two complementary positions we have unbroken circles. The same phenomena precisely are exhibited by this disk of chilled glass in parallel light, the gradually decreasing elasticity of the glass as we recede from the centre having the same effect as the increasing convergence of the rays has in the calcite. Now it is pretty easy to explain this phenomenon to a student by such a diagram as this (fig. 6) representing our crystal or glass with the Nicols crossed. The circularly-polarized ray we know is, on entering the glass, decomposed into its two plane-polarized components, of which one (let us suppose that denoted by the arrow-heads) is retarded a quarter of a wave. But the calcite or glass, beside this, itself also retards either the radial or the tangential vibration more than the other component—in calcite the radial. Taking, then, any originally-circular ring caused by the calcite retardations alone, we see that in two opposite quadrants the $\frac{1}{4}\lambda$ plate retards the radial vibrations a further quarter-wave, while in the alternate quadrants it accelerates them a quarter-wave. The result must

obviously be a half-wave dislocation. As I have just observed, such a diagram sufficiently explains it all; but it seems to me better actually to represent it optically, by introducing the composite quarter-wave plate, with its planes at 45° with the plane of polarization, before a film ground concave to show Newton's rings. Here we have, in an analogous way, in opposite quadrants retarded one of the component vibrations a quarter-wave before entering the selenite, while in the alternate quadrants we retard the other component; and we get similar dislocations. Again, letting the concave selenite come first, and superposing a $\frac{1}{4}\lambda$ plate cut in quadrants with their planes alternately horizontal and vertical, we now have the contracting and expanding quadrants, with the perfect circles in two positions, as in the calcite. We may make the demonstration complete by reversing the process, and superposing our last composite $\frac{1}{4}\lambda$ plate on the disk of chilled glass*. We now are applying in each quadrant all the retardations equally to either the tangential or radial vibrations; and hence the rings remain perfectly concentric, while they expand or contract as the analyzer is rotated: there is no dislocation at all. Finally, either the quadrant or 12-sector $\frac{1}{4}\lambda$ plate superposed on this *square* of chilled glass gives us a very beautiful demonstration that the dislocation of the crystal rings is entirely due to the $\frac{1}{4}\lambda$ plate retarding one component ray in the crystal on one side of the plane of polarization or that at right angles to it, and accelerating the same component on the other side of those planes. Here we have the square perpendicularly adjusted, with the composite plate superposed. When the analyzer is rotated, the reversal of the sectors on the lines of the black cross keeps the figure symmetrical, as in the last experiment. But you observe that the *diagonals* of the square are covered, each by a single plate or sector; and a mere glance at the screen makes it obvious that if, *in this position*, these diagonals were covered, as the black cross now is, by the junction-line between two contrary sectors, they would be dislocated, the colours on one

* In private experiment we can of course do this with a plate of calcite: but in a projecting instrument it is rather difficult to ensure the precise axial coincidence of all the arrangements with the axis of the convergent light, without which the experiment fails.

side of the line approaching the centre, and those on the other receding when we rotate the analyzer. But we will now bring these diagonals of the square into the planes of polarizer and analyzer crossed, and superpose the sectors again upon the glass, junction-lines now covering the diagonals. You observe that the state of things is exactly reversed; and the contrary sectors now do keep the figure symmetrical on each side of the diagonals, while, on the other hand, the single $\frac{1}{4}\lambda$ plates which now cover the bisecting diameters of the square preserve the symmetry there also. It is not necessary to add details of explanation which will be familiar to all.

Allow me next to illustrate the beautiful phenomena of crossed films of mica in highly convergent light, such as will take in biaxial angles of, say, 50°. Our starting-point will be Norremberg's beautiful discovery, worked out entirely from theory, that by crossing films of biaxial mica of gradually increasing number and proportionately diminished thickness, there was a gradual approach to the rings and cross of a uniaxial crystal. He found three wave-lengths of retardation the best approximate unit. Here is a single plate of mica—the ordinary biaxial lemniscates; and here are two such plates crossed at right angles—the ordinary figure of a "crossed" crystal, in which we get the black cross. With four plates crossed we get the first approach towards rings, each of the "eyes" being now bisected by a straight fringe placed as a tangent to the figure. Norremberg's next preparation was eight films crossed; but I add one of six, which gives a single perfect though nearly square ring, while eight films give two rings. Twelve give three rings and signs of a fourth; while twenty-four, as you see, are absolutely undistinguishable from a calcite. The whole series will be thus:—

$$\frac{1}{3\lambda},\ \frac{2}{3\lambda},\ \frac{4}{\frac{3}{4}\lambda},\ \frac{6}{\frac{1}{2}\lambda},\ \frac{8}{\frac{3}{8}\lambda},\ \frac{12}{\frac{1}{4}\lambda},\ \frac{24}{\frac{1}{8}\lambda}.$$

Now there is no necessity for an *exact* total thickness of three wave-lengths in constructing this series; but an approximation to it is necessary, to preserve the gradation of the phenomena and the gradual passage to the uniaxial figure. So far Norremberg ascertained; but he does not seem to have carried his experiments with mica any further. Let us now do so.

The eight films gave us two rings, the outer one squarish in figure. But if we combine eight very *thin* films (say $\frac{1}{8}\lambda$ thick, as in this preparation), you observe that we get perfectly circular rings at once ; and in fact even four very thin films will give them ; and twelve thin films give us quite fine circles. Now, on the other hand, let us employ four and eight *thick* films—in this case over 1λ thick (we thus more than double Norremberg's thicknesses); and observe that the rings now have altogether disappeared, and the curved fringes are all turned the reverse way, their convex sides to the centre. The same thing is still more evident in this splendid figure, produced by twelve crossed films $\frac{3}{4}\lambda$ in thickness. We see easily enough that it must be so, if we follow in our minds the decompositions and recompositions of the vibrations in traversing the successive films ; but it is very interesting to notice how, with the same number of micas crossed in exactly the same way, but of different thicknesses, the phenomena appear actually reversed in character. Having seen this, we abandon simply crossed films, and the following will be composed of films superposed at angles of both 90° and 45°. Here another cause of variety comes into play, since all films whose thickness contains an odd $\frac{1}{4}\lambda$, when superposed at 45° will circularly polarize the light. Moreover we also know that if two such films are superposed at an angle of 45°, the effect is to rotate the plane of polarization itself (as shown for instance by the rotation of a calcite cross) 45° from the original plane. Hence the variety and scope for combination here are endless, the phenomena always being beautiful; but I must only show you a very few of such preparations. The first four are all composed of films $\frac{1}{2}\lambda$ in thickness, and each contains the same number of twelve films, and the lines show the successive positions of the mica "axis." In the first they are

| ╲ | — ╲ — | ╲ | — ╲ —

Thus all the diagonal axes lie the same way. Now this second preparation has the very same individual films differently placed, thus:—

| ╲ — | ╲ — | ╱ — | ╱ —

You see the total difference in effect produced by the difference in crossing.

The next one is thus arranged:—

| | — | | — | = | =

This is an interesting combination, because the wave-decompositions indicate that the light should be nearly extinguished when the Nicols are crossed, not only in the original black cross, but also along the diagonals between. You see that it is so; but this result is still more completely brought out by the next preparation,

| — | — | — — | — | — |

where we get a nearly perfectly black square crossed by nearly black diagonals as well as by the black cross. The next set of five are all built of films one wave in thickness, as follows:—

No. 1. 8 micas | — | × | — |
2. 10 „ + + × + +
3. 12 „ + + × × + +
4. 8 „ + × + ×
5. 8 „ | ╱ — ╲ | ╱ — ╲ (*i. e.* successively rotated 45°).

This last figure is interesting, because we can see that the result must be some polygonal or roughly circular central figure with some sort of a cross, surrounded by eight detached figures or eyes. I am sorry I cannot work this out mathematically; but with whole-wave and somewhat greater retardations it is pretty easy to trace it in one's own mind. It is so, as you perceive; and you also see that the preparation and figure can be rotated without very sensible change, which also follows from theory, and is a somewhat remarkable result, after what we saw at the commencement, with such thick films. And now, to show the effect of thickness, here is a precisely similar preparation of eight films superposed at a successively rotated angle of 45°, but built up of $\frac{3}{4}\lambda$ films. Circular polarization here comes into play; and the effect is totally different in every way. The last of these crossed micas is built up of twelve $\frac{3}{4}\lambda$ films, thus—

× + + + + ×

You see the total difference in figure from any thing before, and the scope for endless variety, which I must not further pursue.

Still more beautiful, but perhaps less interesting, are the

combinations of mica- and selenite-films discovered experimentally by Norremberg. As he observes, if we call the three axes of elasticity in any crystal x, y, z, then selenite-films contain x and z, while mica on the other hand contains y and z; and it is easy to see that if preparations are built up of both elements, very fine coloured fringes must result, differing very greatly in character according to whether the x of the selenite is parallel to the z of the mica or crosses it. As far as I remember, however, Norremberg and Reusch seem to have said that the characters of the fringes defy all prediction. This is perhaps hardly true, even apart from mathematical analysis, which I am unable to give, and which the mere beauty of these combinations is scarcely worth. For it is easy to perceive that if a single selenite be placed between two thick micas, we must have very nearly the usual biaxial figure, with some little modification in the eyes or rings, but chiefly distinguished from the simple mica by rich colour. That is so here. But if we alternate several parallel selenites between parallel micas of less thickness, so as to give the selenite functions of elasticity more comparative influence, then it is evident that the modified lemniscate curves, or what is still traceable of them, must be either brought nearer together or more widely separated, and that we shall thus obtain curved fringes having their approximate origin in the original optic axes of the mica, but reversed in character according as the x of the selenite crosses or is parallel to the mica z. This is the simplest analysis I can give, and it follows on consideration from Norremberg's data. Here are two such preparations, in each of which four selenites are alternated between five $\frac{1}{2}\lambda$ micas. In the first the modified lemniscates are wider apart than in the mica, but the resulting fringes originate approximately in the mica axes. In the second the selenites are at right angles with their former position, and the fringes still centre in the axes, but the curves are reversed; and the resulting "palm-tree" pattern is perhaps one of the most beautiful, both in colour and figure, which it is possible to behold.

The few other preparations here are built up of either four or six ternary elements constructed on Norremberg's system, each consisting of two parallel micas, with a selenite between

them either crossed or parallel. In the first you readily recognize the "palm-tree" character of the last figure, "crossed." It is needless to describe the others; for here, too, variety is boundless; but I purposely reserve for the last, two combinations composed of exactly the same arrangements of both mica and selenite, and all the micas the same ($\frac{1}{2}\lambda$) thickness, but the selenite films in one slightly thicker than in the other. The difference in effect is purposely not so great as to prevent your recognizing the same general figure in both, but is still conspicuous and interesting. Let me, in conclusion, hope that the beauty of these preparations constructed after Norremberg's method, and the facility with which they can be prepared, may make them better known.

[At the conclusion of the paper Mr. Wright described and exhibited an adaptation to the microscope by Messrs. Swift and Son, by the aid of which all the preparations and crystals requiring highly convergent light could be shown on the stage of any microscope constructed with a draw-tube.]

XXII. *On a Method of Measuring Electrical Resistances with a Constant Current.* *By* SHELFORD BIDWELL, *M.A., LL.B.**

It sometimes happens that the resistance of a body appears to depend upon the strength of the current which traverses it. Thus the resistance of the carbon filament of an incandescent lamp may be several ohms lower when tested with a strong current than it is with a weak one. In this case there is little doubt that the difference is due only indirectly to the current itself, and is in fact caused by the heat which the stronger current develops, and which, even when the circuit is closed only for a moment, may produce considerable effect upon the conductivity of the filament. Again Prof. Adams, at an early stage of his well-known experiments with selenium, found that, on increasing the strength of the current through the selenium, there was a diminution in its resistance †. The same is the case with the mixtures of sulphur and carbon which I described in a previous

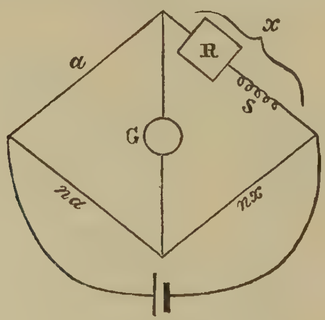
* Read March 10, 1883.

† Phil. Trans. vol. clxvii. pp. 319, 342.

communication *, and to a very much greater degree with loose contacts of carbon or metal, such as are used in the microphone. For example, a carbon pencil being arranged so as to rest at right angles upon another with a pressure of ·05 grm., the resistance at the point of contact was found to be 11·02 ohms with a current of ·1 ampere, and 68 ohms with ·001 ampère; and when cylinders of bismuth were substituted for the carbon, the resistances with the same currents as before were 5 ohms and 182 ohms respectively.

Without assuming that the resistances in these and similar cases are altogether true resistances, it is nevertheless sometimes convenient to treat them as such; and for purposes of comparison it is clearly necessary that currents of known or constant strength should be used in their measurement. When the Wheatstone's bridge is employed in the usual manner, the current passing through the unknown resistance will, of course, vary with the magnitude of this resistance, being smaller when it is high than when it is low; but by a very simple modification of the common arrangement, which I have used extensively during the last year, it is easy to ensure having currents of uniform strength throughout a series of measurements.

In the figure, x, nx, a, na are the four arms of a Wheatstone's bridge, S is the unknown resistance, and R is a box of



resistance coils which is inserted in the same arm. If E denote the electromotive force of the battery, B its internal resistance, and C the current which passes through the arm

* Proc. Phys. Soc. vol. v. p. 90.

containing S, then, when there is a balance,

$$C=\frac{n}{n+1}\times\frac{E}{B+\frac{n(a+x)^2}{a+x+n(a+x)}}$$

$$=\frac{nE}{(n+1)B+n(a+x)}.$$

From this expression we can find what value x must have in order that the current through the unknown resistance may be of any definite strength. Having determined this value, we insert resistance equal to n times its amount in the arm nx, and adjust the resistance in the box R until a balance is obtained. We then know that the resistance of R + S is equal to x; that the resistance required to be measured, S, is equal to that of the arm x less the resistance employed in R; and that a current of the desired strength, C, is passing through it. A second unknown resistance may now be substituted and measured as before, simply by altering the resistance of R, with the certainty that when there is a balance the current is of the same strength as in the former case. The resistance nx remains unchanged throughout. It is of course necessary so to choose the values of a, n, and E that x may be greater than the resistance to be measured; and it is generally desirable that the resistance of the whole bridge should be made as high as conveniently possible.

The great advantage of this method over others that suggest themselves lies in the fact that, since it is never necessary to close the circuit for more than a moment, the electromotive force and resistance of the battery remain sensibly constant during a long course of experiments.

XXIII. *The Resistance of the Electric Arc.*

By Professors W. E. AYRTON, *F.R.S., and* JOHN PERRY, *M.E.**

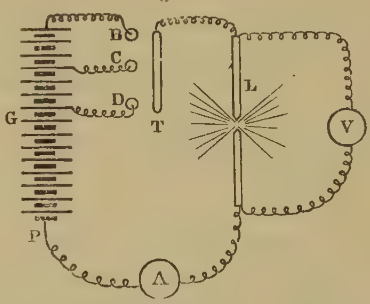
ONE of the results of the elaborate set of experiments on the Electric Light conducted in 1878 by the late Mr. Schwendler was the conclusion to which he came, that the supposition that the resistance of an arc of constant length

* Read December 9, 1882.

was inversely proportional to the current which passed through it was highly probable. His experiments, however, were not sufficient to absolutely determine this point; and it has therefore appeared to us important to obtain further information on the subject, which, with the assistance of the students working in our laboratory, we have from time to time done with the following results.

1. The method employed by us in the first instance was as follows :—A number of Grove cells, G, were arranged in series (fig. 1); and one pole P was connected through an ammeter A with one carbon of the electric light L, the other carbon of which was attached to a mercury trough T, which, by means of a metallic bridge-piece, could be connected with any one of the mercury-cups B, C, D, each of which was permanently electrically connected with the terminal of a different number of cells. The two carbons were also connected with the terminals of a voltmeter V, by means of which the difference of potentials between the carbons at any moment could be determined. The experiment was made thus :—The bridge-piece was put into D, and the carbons by means of a rack adjustment separated until a good steady light was obtained, when readings of the ammeter and voltmeter were taken. A second bridge-piece was now put into C and that in D quickly withdrawn, the effect being to suddenly increase the number of cells in circuit producing the light, increasing therefore the current without interrupting it, and without changing the distance between the carbons, as the lamp had no automatic adjusting arrangement. Readings of A and V were then quickly taken and the operation reversed—that is, the bridge-piece put into D and that in C quickly withdrawn, which had the effect of again reducing the current; and if the change back again were effected not long after the first, the carbons were not sufficiently burnt away by the stronger current to make the light go out when the

Fig. 1.



current was reduced, so that a third set of readings of A and V could be taken. In this way, for the same distance between the carbons two readings of the lower current and its corresponding carbon difference of potentials, and one intermediate reading of the higher current with its carbon difference of potentials, were obtained. The whole experiment was now repeated with the cells P C and P B instead of P D and P C. The following is a sample of the results obtained from a number of tests with 30, 40 and 50 Grove cells :—

Number of cells.	Current, in ampères.	Difference of potentials between carbon, in volts.	Work in foot-pounds per second in arc.
30	6·52	30·4	146·2
40	10·16	30·4	227·8
50	11·92	30·4	267·2

The last current is therefore nearly double the first, but the difference of potentials between the carbons is not materially altered by the increase of the current and the light.

Subsequently a large number of experiments were made using a Brush dynamo in place of the Grove cells, and increasing and diminishing the current by suddenly increasing and diminishing the resistance in circuit without stopping the current.

In the earlier experiments for each current its value was read on the ammeter as well as the difference of potentials between the carbons on the voltmeter; but since even with a very dead-beat ammeter some little time must elapse when the currents are alternately doubled and halved by taking out and inserting resistance in circuit, and since even with a slight delay the stronger current burns away the carbon points very rapidly, and so makes the distance between them for the stronger currents greater than for the weaker, it was thought better in the later experiments merely to take readings of the voltmeter when the resistance was altered backwards and forwards sufficiently to alternately treble and diminish to one third the current as shown by the earlier experiments. The following are samples of the readings of the voltmeter, the distance between the carbons in each case being fixed and the current alternately trebled and diminished to one third.

Current approximately.	Difference of potentials between carbons, in volts.	
1	26·5	
3	26·5	
1	24·5	
3	25·5	
1	26·5	Distance between carbons re-adjusted.
3	26·5	
1	25·5	
3	26·5	
3	32	A somewhat greater distance between the carbons.
1	30	
3	39·5	
1	30	
3	34	
1	34	
3	36	
1	38	
3	30	
1	28	
3	30	
3	30	Distance between carbons re-adjusted.
1	28	
3	30	
1	28	
3	30	

It would appear therefore that for a fixed distance between the carbons the difference of potentials necessary to maintain the arc is nearly but not quite independent of the current, the electromotive force requiring to be slightly increased when the current is very much increased.

2. The second part of the investigation was for the purpose of ascertaining in what way the differences of potentials between the carbons varied with the length of the arc when the current was kept constant. For this purpose the arc was projected on a distant scale by means of a lens, the magnifying-power of the arrangement being calculated, first, by comparing the distance between the scale and the lens with the distance between the arc and the lens, secondly by putting close to the arc a piece of carbon of known thickness and measuring quickly the thickness of its image as projected on the distant screen, before the piece of carbon had time to burn.

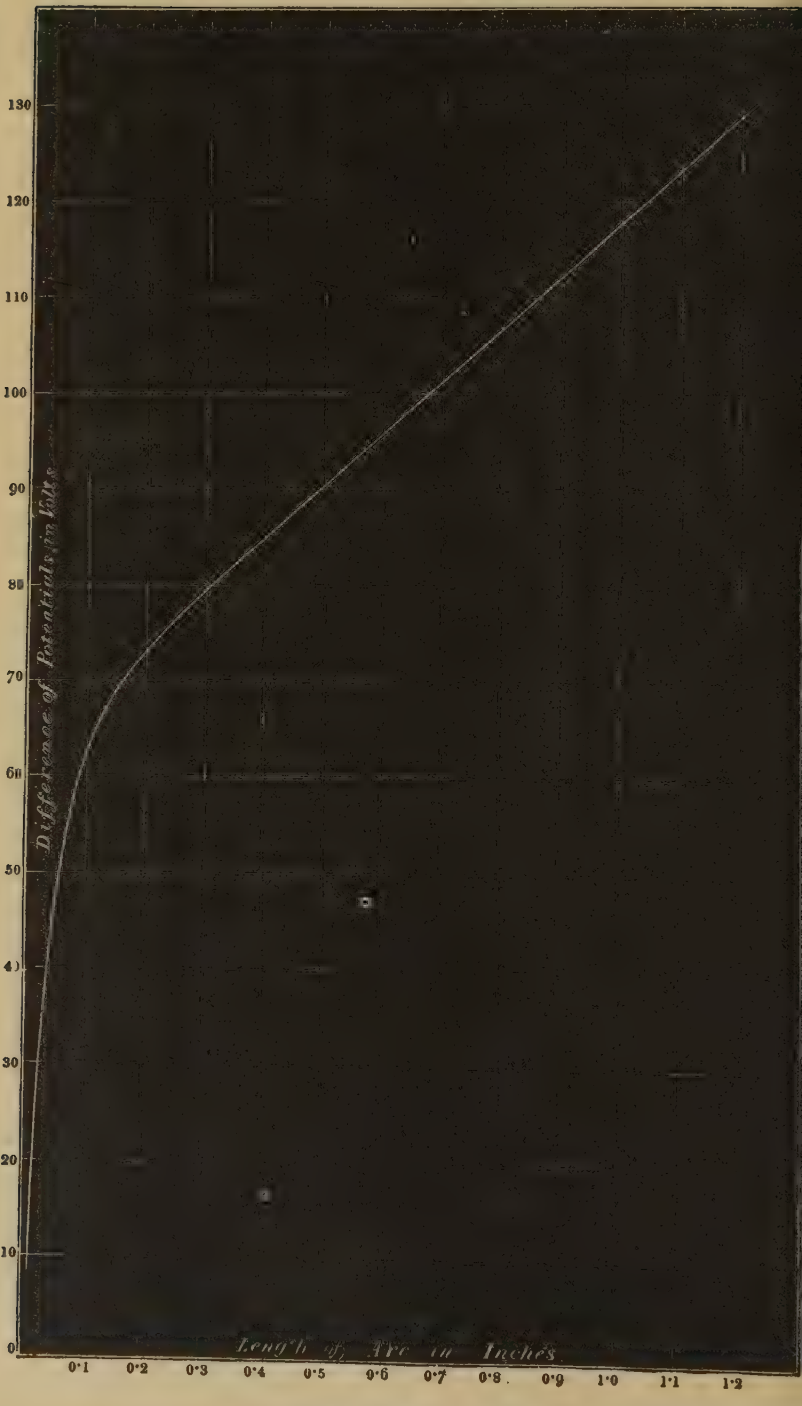
For each set of experiments a particular current was decided on: the carbons were put successively at different distances apart and the resistance in the circuit varied until the prearranged current was flowing through the arc, when instantly the actual projected distance between the carbons on the screen was read off and the difference of potentials between the carbons in volts; or the resistance in the circuit external to the lamp could be left fixed, and the carbons gradually withdrawn until the prearranged current was flowing through the arc, when, as before, the projected length of arc and the difference of potentials between the carbons was read off. A large number of experiments were made in this way with a Brush machine for currents varying between 5·5 and 10·4 ampères, the distances between the carbon points from 0 to one and a quarter inch, and the difference of potential varying from 0 to 140 volts, the carbons being 0·24 inch thick. The result when plotted gave a curve similar to that shown in fig. 2 (p. 202), horizontal distances representing distances between the carbon points, and vertical distances the difference of potentials between the carbons. For all the currents approximately the same curve was obtained—a result to be expected, seeing that the first investigation showed that the difference of potentials between two carbons necessary to produce an arc depended almost entirely on the distance between them, and hardly at all on the strength of the current. The equation to the curve we find to be approximately as follows:—

$$E=63+55a-63\times10^{-10a},$$

where E is the difference of potentials in volts between the carbons, and a the distance between their points in inches. It will be seen that at first the difference of potentials necessary to maintain the arc increases rapidly with the distance, and that at a distance of about one tenth of an inch it is about 60 volts. From this the curve bends rapidly up to a point corresponding with a distance between the carbons of about one quarter of an inch; and for greater distances between the carbons than one quarter of an inch, the increase of difference of potentials becomes nearly proportional to the increase of distance, being about 54 volts per inch increase.

This law is very like that found by Mr. C. F. Varley for the discharge through a vacuum-tube, which was that the

Fig. 2.—The Resistance of the Electric Arc.



current was proportional to the difference of potentials minus a constant; for this is equivalent to saying that, *cæteris paribus*, the difference of potentials necessary to produce a fixed current is proportional to the length of the tube plus a constant. The curve we have obtained is also strikingly like that obtained by Drs. W. De La Rue and Hugo Müller for the connexion between the electromotive force and the distance across which it would send a spark*. These gentlemen also made experiments on the electric arc with their large battery; but we do not find recorded any results with carbon points. On page 185 of the reprint from the 'Philosophical Transactions' of the account of their researches, the result of an experiment in air between two brass points is given; but according to that, when the arc was half an inch in length the difference of potentials between the brass points was that of 657 of their cells, or about 700 volts. How far the very high electromotive force found by Drs. W. De La Rue and Hugo Müller, to be necessary in this case, arose from a combination of the material employed for the electrodes and the smallness of the diameter of the brass electrodes, or whether the law that "the electromotive force necessary to maintain an arc depends mainly on the length of the arc and hardly at all on the strength of the current" fails when the current is below a certain small limit, we are unable to say; but of course both the diameter of the brass electrodes they employed and the strength of the current that was passing (0·025 ampère) in the arc was very much less than that used in any ordinary electric light, and to which the experiments of Mr. Schwendler and ourselves especially refer. It is very probable that the difference in the material of the electrodes has mainly to do with the difference between their results and ours; and we think it very probable that with very soft carbons an arc of a given length could be maintained with a much less difference of potentials than that found by us, since it would be more easy for a shower of carbon particles to be maintained between the ends of the carbons.

We have used as the title of this short communication the Resistance of the Electric Arc; but we are perfectly aware of

* Page 32 of the Reprint of "Experimental Researches on the Electric Discharge," Phil. Trans. part i. vol. 169.

the objections to this expression. How far the opposition to the passage of the currents in an electric arc is due to pure resistance, and how far to an opposing electromotive force, is up to the present time by no means certain. That there is some opposing electromotive force, seeing that mechanical disintegration of the carbon and transporting of its particles occurs, is, as was pointed out some years ago by Edlund, almost certain; but seeing that this opposing electromotive force ceases to exist with the extinction of the arc, and probably varies as the pure resistance varies, and further remembering that an opposing electromotive force which has no existence apart from a combined resistance acts in any electrical test exactly as a resistance, it must be always very difficult experimentally to separate them. All of course that we can measure electrically is the difference of potential between the carbons and the current passing between them; and this is what we have been measuring all through these two investigations.

It may be here noted that in all probability the conduction from particle to particle in a microphone is of the nature of a small electric arc, or, rather, perhaps a convective discharge, seeing that the resistance in a microphone varies with the current used to measure it; indeed it is probable, when the pieces of carbon or other material employed, are so pressed together that close intimacy of contact of the particles makes the resistance tolerably independent of the current, that then the pieces of carbon will not act as a microphone at all.

We have to thank Messrs. W. Atkinson and Lt. B. Atkinson, two of our students, for much assistance rendered us in these experiments.

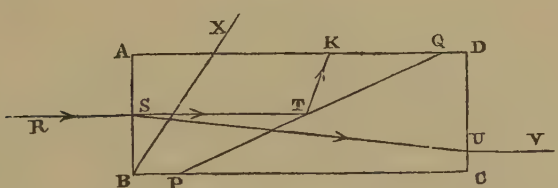
XXIV. *On Polarizing Prisms. By* R. T. Glazebrook, *M.A., F.R.S., Fellow and Lecturer of Trinity College, Demonstrator in the Cavendish Laboratory, Cambridge* *.

In a paper on Nicol's prism (Phil. Mag. vol. x. 1880) I have considered some of the defects of Nicol's prism as a means of producing plane-polarized light. In the present

* Read April 14, 1883.

paper I propose to describe a form of polarizing prism free from most of these. For many purposes, one of the great objections to Nicol's prism is the lateral displacement produced by it in the image of an object viewed through it. If we place a Nicol before the object-glass of a telescope, on turning the Nicol round its axis the image moves across the field. This has been remedied somewhat by cutting prisms with their ends at right angles to their length, and making the angle between the normal to the face on which the incident light falls and the plane of Canada balsam such that the ordinary ray is totally reflected there while the extraordinary ray passes through. But this is not entirely successful; for let A B C D (fig. 1) be a section of such a prism by a plane parallel to the edge B C and at right angles to the Canada balsam. Let P Q be the trace of the balsam. In an ordinary Nicol's prism A B would be inclined at about 74° to A D, and P Q

Fig. 1.



would be at right angles to A B, A D and B C being parallel to edges of the rhomb of spar. In the case now being considered, A B is at right angles to B C, B C being still parallel to a rhombic edge.

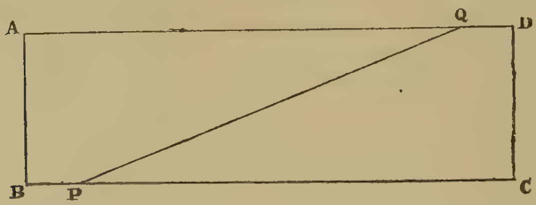
Consider a ray R S incident normally on A B. The ordinary ray S T enters the spar without deviation, but is reflected by the balsam at T in direction T K; the extraordinary ray is refracted at the face A B in direction T U, and turned from its original path in virtue of the extraordinary refraction. It emerges along U V parallel to its original direction, but displaced to one side, so that the extraordinary image of the object seen is displaced to one side by the passage of the light through the spar.

In the prism considered in fig. 1, the optic axis lies in the plane of the paper, making an angle of 57° 30′ with B C.

Suppose now we cut a rectangular parallelepipedon from a piece of spar, in such a way that two of its faces are at right angles to the optic axis while the other four are parallel to it.

Let A B C D (fig. 2) be a section of the solid by a plane also at right angles to the optic axis, and therefore parallel to two faces and at right angles to the other four; and suppose that

Fig. 2.

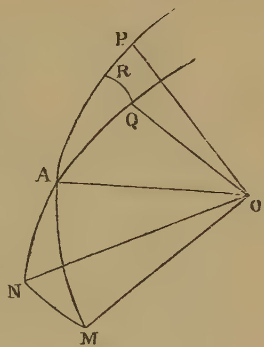


B C is about three times A B. Let P Q be inclined at about 20° to B C, and suppose the prism cut in two by a plane at right angles to the paper and passing through to P Q. Then let the faces of section be polished, and cemented together with Canada basam. The optic axis will be at right angles to the plane of the paper, and the section of the wave-surface by that plane will be two circles of radii A and C, these being the ordinary and extraordinary wave-velocities respectively. Hence a ray falling on the face A B in any direction in the plane of the paper will be divided into two, which will both undergo ordinary refraction, so that if the incident ray be normal to the face A B, the extraordinary and ordinary rays in the prism will coincide in direction, both being normal to the same face. The extraordinary ray is not deviated by the refraction; so that no lateral displacement of the extraordinary image is produced by the prism. The ordinary ray is incident at about 70° on the face P Q; it is therefore totally reflected, and the emergent light is plane-polarized. The prism differs from one described by Prof. S. P. Thompson (Phil. Mag. Nov. 1881) only in the fact that its ends are normal to its length instead of being inclined obliquely to it. But this form of prism has other and more important advantages.

Let O M, O N (fig. 3) be two extraordinary wave-normals

and O A the optic axis. Pass a plane M O A through O M and O A, and in this plane draw O P at right angle to O M; then O P is the direction of vibration in the wave which travels along O M. Similarly, if N O A be a plane through O N and O A, and O Q a line in it at right angles to O N, O Q is the direction of vibration for the wave along O N; and it may happen, clearly, that O P and O Q are inclined to one another at a large angle even when O M and O N are close together. Suppose, then, that the extraordinary pencil of wave-normals

Fig. 3.



which is traversing the spar is slightly conical, and that O N, O M are two of the wave-normals; the planes of polarization are inclined to each other at an angle equal to P O Q; and this may be considerable. Or, again, suppose that we have a polarized pencil of parallel wave-normals incident on the prism. We determine the position of their plane of polarization by turning the prism until no light passes through. Suppose that, when this is the case, the incident light is parallel to O M. Now let the plane of polarization of the incident light be rotated, and suppose we wish to measure this rotation; we turn the prism until the light is again quenched. Theoretically the axis round which the prism has been turned should be parallel to O M. In practice it is difficult to ensure this; and in general the direction of the wave-normal relatively to the optic axis will be changed, and may now be O N say. But since the planes of polarization of the waves along O M and O N are different, the angle through which the prism has been turned will not be the angle through which the plane of polarization of the incident light has moved.

Now Nicol's prism is so cut that the angle between the planes of polarization of two waves inclined to each other at but a small angle as they traverse the crystal is considerable. If, then, a slightly conical pencil traverse the prism, the angles between the planes of polarization of the different waves are considerable; or if a parallel pencil traverse the prism inclined

at but a small angle to the axis of rotation, and the plane of polarization of this beam be rotated, that rotation will differ considerably from the angle through which the prism has to be turned to reestablish blackness.

In our figure the wave along O M is polarized in a plane at right angles to O P, that along O N in a plane at right angles to O Q. Consider now a conical pencil of wave-normals in air: it is clearly impossible for it to be plane-polarized, if by plane polarization we mean that the directions of vibration are parallel to the same line; for we cannot have a series of lines touching a sphere all parallel to the same line. Such a pencil, however, may be said to be most nearly plane-polarized when all the directions of vibration are parallel to the same plane; and this plane will be that which passes through the axis of the pencil and the direction of vibration for the wave-normal which coincides with the axis. For if this be the case, the whole of the pencil can pass unaltered either as an ordinary or extraordinary wave through a piece of spar on which its axis falls normally, provided that the optic axis of the spar be respectively either at right angles to or parallel to the plane in question. Using "plane polarization" in this sense, we proceed to consider when a conical pencil of given vertical angle travelling in a piece of uniaxal crystal is most nearly plane-polarized.

Now let O M (fig. 3) be the axis of the pencil, and O P the direction of vibration for the light travelling along O M, and let O N be any other wave-normal. According to the above statement, the conical pencil will be most nearly plane-polarized if the vibration travelling along O N is parallel to the plane P O M. If, however, the pencil be travelling in a crystal, it is clearly impossible in general for the displacement along O N to be parallel to this plane. For let O A be the optic axis; O A lies in the plane M O P. Pass a plane through O A, O N, and in it draw O Q at right angles to O N; O Q is the direction of displacement which travels along O N, and O Q is not parallel to the plane P O M.

We can resolve the displacement along O Q into two, in and perpendicular to the plane P O M. The light then will be most nearly plane-polarized when the average intensity of the

vibrations normal to this plane is least; and it remains to find the condition for this.

In fig. 3 let QR be perpendicular to AP. Let AM $=\alpha$, NM $=\beta$, AN $=\theta$, AMN $=\phi$. Let ρ be the amplitude of the displacement along OQ. The displacement normal to the plane PAM is $\rho\sin$ QR, and the intensity of the wave is proportional to $\rho^2 \sin^2$ QR.

We are to consider a hollow conical pencil with OM as axis. An element of such a pencil at N will be $\sin\beta d\phi$; and the total energy in the pencil, so far as it depends on the displacement normal to the plane PAM, is

$$2\int_0^\pi \rho^2 \sin\beta \sin^2 \mathrm{QR}\, d\phi.$$

Now

$$\mathrm{AQ} = \frac{\pi}{2} - \theta,$$

$$\sin \mathrm{QK} = \sin \mathrm{AQ} \sin \mathrm{RAQ}$$

$$= \cos\theta \sin \mathrm{NAM},$$

$$\sin \mathrm{NAM} = \frac{\sin\phi \sin\beta}{\sin\theta};$$

$$\therefore \sin \mathrm{QR} = \cot\theta \sin\phi \sin\beta. \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Also

$$\cos\theta = \cos\alpha\cos\beta + \sin\alpha\sin\beta\cos\phi. \quad . \quad . \quad (2)$$

Substituting in the value of sin QR, we find for the energy required the expression

$$2\rho^2 \sin^3\beta \int_0^\pi \frac{\sin^2\phi(\cos\alpha\cos\beta + \sin\alpha\sin\beta\cos\phi)^2 d\phi}{1-(\cos\alpha\cos\beta + \sin\alpha\sin\beta\cos\phi)^2}. \quad . \quad (3)$$

And we require to evaluate this integral.

Let

$$\cos\alpha\cos\beta = a, \quad \sin\alpha\sin\beta = b.$$

Then

$$\int_0^\pi \frac{\sin^2\phi\,(\cos\alpha\cos\beta + \sin\alpha\sin\beta\cos\phi)^2\, d\phi}{1-(\cos\alpha\cos\beta + \sin\alpha\sin\beta\cos\phi)^2}$$

$$= \int_0^\pi \frac{\sin^2\phi(a+b\cos\phi)^2\, d\phi}{1-(a+b\cos\phi)^2}$$

$$= \int_0^\pi \frac{\sin^2\phi}{1-(a+b\cos\phi)^2} d\phi - \int_0^\pi \sin^2\phi\, d\phi.$$

The first term

$$= \tfrac{1}{2}\int_0^\pi \left\{ \frac{\sin^2\phi}{1-(a+b\cos\phi)} + \frac{\sin^2\phi}{1+(a+b\cos\phi)} \right\} d\phi.$$

But

$$\int_0^\pi \frac{\sin^2\phi\, d\phi}{c+d\cos\phi} = \int_0^\pi \frac{(1-\cos^2\phi)\, d\phi}{c+d\cos\phi}$$

$$= \int_0^\pi \left\{ \frac{c}{d^2} - \frac{\cos\phi}{d} - \frac{c^2-d^2}{d^2(c+d\cos\phi)} \right\} d\phi$$

$$= \frac{\pi c}{d^2} - \frac{c^2-d^2}{d^2} \frac{2}{\sqrt{c^2-d^2}} \frac{\pi}{2},$$

if c is $> d$,

$$= \frac{\pi}{d^2}\{c-\sqrt{c^2-d^2}\}.$$

Hence

$$\int_0^\pi \frac{\sin^2\phi}{1-a-b\cos\phi}\, d\phi = \frac{\pi}{b^2}\{1-a-\sqrt{(1-2a+a^2-b^2)}\}; \quad (4)$$

for we can easily show that c is $> d$ in this case. And

$$\int_0^\pi \frac{\sin^2\phi}{1+a+b\cos\phi}\, d\phi = \frac{\pi}{b^2}\{1+a-\sqrt{(1+2a+a^2-b^2)}\}. \quad (5)$$

And the required integral is

$$\frac{\pi}{2b^2}\{2-\sqrt{(1-2a+a^2-b^2)}-\sqrt{(1+2a+a^2-b^2)}-b^2\}. \quad . \quad (6)$$

But

$$a^2-b^2 = \cos^2\alpha\cos^2\beta - \sin^2\alpha\sin^2\beta = \cos^2\alpha + \cos^2\beta - 1.$$

Hence, since the positive sign is to be attached to the roots, we have, if β be $< \alpha$,

Intensity required

$$= \frac{\pi\rho^2\sin\beta}{\sin^2\alpha}\{2-(\cos\beta-\cos\alpha)-(\cos\beta+\cos\alpha)-\sin^2\beta\sin^2\alpha\}$$

$$= \pi\rho^2\sin\beta\,(1-\cos\beta)\left\{\frac{2}{\sin^2\alpha}-(1+\cos\beta)\right\}$$

$$= 4\pi\rho^2\sin\beta\sin^2\frac{\beta}{2}\left\{\operatorname{cosec}^2\alpha - \cos^2\frac{\beta}{2}\right\}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

And if α be $< \beta$,

Intensity

$$= \pi\rho^2\sin\beta\left\{\frac{2}{1+\cos\alpha}-\sin^2\beta\right\} = \pi\rho^2\sin\beta\left\{\sec^2\frac{\alpha}{2}-\sin^2\beta\right\}. \quad (8)$$

In the first case the intensity is clearly least when α is $\frac{\pi}{2}$, its value then being

$$\pi\rho^2 \sin\beta(1-\cos\beta)^2;$$

and in the second case it is least when α is 0, and its value is

$$\pi\rho^2 \sin\beta \cos^2\beta.$$

This second minimum will be greater than the other if

$$\cos\beta \text{ is } > 1-\cos\beta,$$

i. e. if $\qquad \cos\beta$ is $> \frac{1}{2}$,

i. e. if $\qquad \beta$ is $< 60°$.

If, then, a conical pencil whose semi- vertical angle is less than 60° be passing through the spar, the pencil will be most nearly plane-polarized if the axis of the pencil is at right angles to that of the spar.

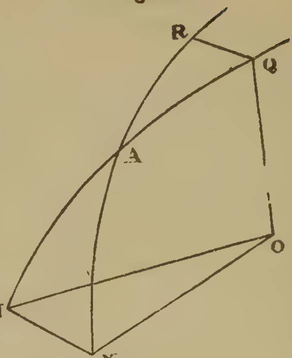
Now if the axis of a conical pencil pass normally through a prism cut as already described, it will be at right angles to the optic axis; and hence the pencil, if its semi- vertical angle be less than 60°, will be more nearly plane-polarized than it would be if the axis occupied any other position. This constitutes a second advantage in favour of the new prism.

Again, suppose we have a parallel pencil of wave-normals in direction ON, and that the axis round which the prism rotates is OX (fig. 4). In our observations we suppose that these two coincide, and work as if the plane of polarization of the emergent light coincided with that of light travelling along OX, thus introducing an error. The amount of this error will depend of course partly on the angle NX (β say), and partly on the angle NXA (ϕ say), OA being the optic axis. If we know β and ϕ we can calculate the error, and could determine the value to be given to XA or α to make it the least possible.

But in practice ϕ may be anything between 0 and 2π, and β anything between 0 and a not very large angle β_1; and the question arises, what value must we assign to α in order that the error produced by any chance values of β and ϕ may most probably be as small as possible? To answer this we require to determine, between these limits for β and ϕ, the

mean of the values, irrespective of sign, of the amplitudes of the vibrations normal to the plane OAX.

Fig. 4



Let O Q be the direction of vibration for the wave-normal O N, and Q R perpendicular on the plane A X. Then the displacement normal to the plane AOX is $\rho \sin QR$; and, with the same notation as before, the average displacement

$$=\frac{\rho}{2\pi}\int \sin\beta \sin\phi \cot\theta \, d\phi. \quad . \quad . \quad . \quad . \quad (9)$$

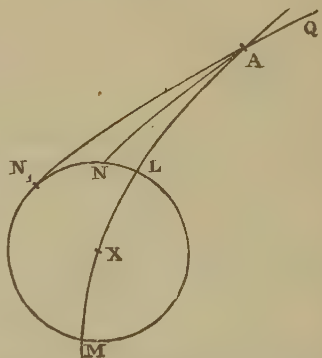
Also $\sin\theta \, d\theta = \sin\alpha \sin\beta \sin\phi \, d\phi.$

Hence average displacement

$$=\frac{\rho}{2\pi \sin\alpha}\int \cos\theta \, d\theta. \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

We must consider now the limits between which the integral is to be taken.

Fig. 5.



Describe a circle (fig. 5) passing through N with X as centre, and cutting A X in L and M, and if possible let N_1 be a point on this circle such that

$$N_1A = \frac{\pi}{2}.$$

Suppose we treat displacements to the right of AX as positive. When N coincides with L,

$$\theta = \alpha - \beta.$$

If N lies between L and N_1, Q is to the right of AX, as in figure, and the displacement is positive. We must therefore integrate for θ from $\alpha - \beta$ to $\frac{\pi}{2}$. But if N lie between N_1 and M, Q will be to the left of AX, and the displacement will be negative. Thus, to get the whole average displacement, irrespective of sign, we have to subtract the value of the integral

from $\frac{\pi}{2}$ to $\alpha+\beta$. If, however, $\alpha+\beta$ is $<\frac{\pi}{2}$, no position such as N_1 can be found, and we have to integrate straight from $\alpha-\beta$ to $\alpha+\beta$. The same is true for positions of N on the other side of AX.

Hence, in the first case, the average displacement normal to the plane is

$$\frac{2\rho}{2\pi\sin\alpha}\left\{\int_{\alpha-\beta}^{\frac{\pi}{2}}\cos\theta\,d\theta-\int_{\frac{\pi}{2}}^{\alpha+\beta}\cos\theta\,d\theta\right\}$$

$$=\frac{\rho}{\pi\sin\alpha}\{1-\sin(\alpha-\beta)+1-\sin(\alpha+\beta)\}$$

$$=\frac{2\rho}{\pi}\left\{\frac{1}{\sin\alpha}-\cos\beta\right\}$$

$$=\frac{2\rho}{\pi}\{\operatorname{cosec}\alpha-\cos\beta\}. \quad . \quad . \quad . \quad . \quad . \quad (11)$$

And in the second case it is

$$\frac{2\rho}{2\pi\sin\alpha}\int_{\alpha-\beta}^{\alpha+\beta}\cos\theta\,d\theta=\frac{\rho}{\pi\sin\alpha}\{\sin(\alpha+\beta)-\sin(\alpha-\beta)\}$$

$$=\frac{2\rho}{\pi}\cot\alpha\sin\beta. \quad . \quad . \quad . \quad . \quad (12)$$

The first is clearly least when α is $\frac{\pi}{2}$; the second decreases as α increases, but has no minimum; for after a time we should reach a point at which $\alpha+\beta$ became equal to $\frac{\pi}{2}$, and then the limits would require changing: for this value, of course, the two integrals are the same.

Thus the average displacement normal to the plane OAX is least when OX is at right angles to the optic axis, and hence the average error in the position of the plane of polarization is least also. The average displacement just calculated is of course that for a given value of β. If we require the average for any value of β between 0 and β_1, we must multiply our expressions (11) and (12) by $d\beta$, and, integrating from 0 to β_1, divide the result by β_1.

To show the difference in this respect between the new prism and Nicol's, let us calculate the displacement normal to the plane AOX in the two cases, supposing the value of β is 5°.

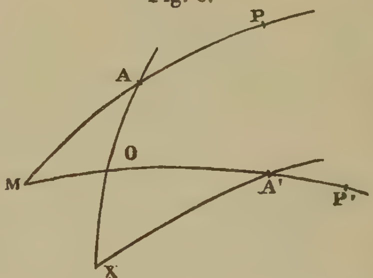
In Nicol's prism, $\alpha = 63° \ 30'$; so that $\alpha + \beta$ is less than $\frac{\pi}{2}$, and the second formula (12) must be taken. The ratio of the two displacements is therefore

$$\frac{\cot 63° \ 30' \sin \beta}{1 - \cos \beta} = \cot 63° \ 30' \cot \frac{\beta}{2};$$

and substituting the value $\beta = 5°$, this comes to 434 : 39, or about 11 to 1. Thus the average error in the position of the plane of polarization as determined by the new prism will be about one eleventh of that which would be produced by the same errors of adjustment with a Nicol's prism; while the amount of light polarized out of the proper plane will be less than one per cent. of that which would be produced by a Nicol.

Again, suppose the prism is turned through an angle ω about OX (fig. 6), and let us inquire what is the angle through which the plane of polarization of the emergent light is rotated. Let O A′ be the new position of the optic axis. Join M A, M A′, and in them take points P, P′ such that $MP = MP' = \frac{\pi}{2}$. O P, O P′

Fig. 6.



are the directions of vibration for the waves travelling along O M in the two positions of the prism respectively. The angle through which the plane of polarization has been turned is P P′ or P M P′, that through which the prism has been turned is A X A′; and we require to investigate the conditions under which the average difference between these two for all possible positions of M within a certain distance, β_1 say, of X.

Now we have seen already that if the axis of rotation be at right angles to the optic axis, the average error produced in the determination of the position of the plane of polarization for each of the two positions of the prism will be a minimum; and hence it follows that the average error in the angle between these two positions is a minimum also.

All these results, of course, hold only for the position of the

plane of polarization of the light when in the crystal, and will be modified by the refraction that takes place as the waves emerge into the air. But since the ends of the prism are normal to its length, for all the waves considered the incidence is very nearly direct, and the change produced by refraction in the position of the plane of polarization is very small indeed.

Thus a prism cut as described possesses the following advantages over Nicol's prism:—

1. There is no lateral dispacement in the apparent position of an object viewed through it.

2. A conical pencil whose axis passes directly through is more nearly plane-polarized than would be the case if the axis of the prism were related to that of the spar in any other manner.

3. If the direction of the wave-normal within the prism does not quite coincide with the axis of rotation, the average error in the position of the plane of polarization is less than for any other method of cutting.

I hope shortly to have some prisms cut by Mr. Hilger in this manner, and to test by means of them the theoretical conclusions arrived at in the paper.

Note added April 26th.

If the plane of section PQ be inclined to BC at an angle of 20°, as in fig. 2, the angular aperture of the field will be small, only about 10°, and it will be necessary that all the light traversing the prism should be very nearly parallel to BC. The aperture may be increased up to about 20° by lengthening the prism considerably and decreasing the angle between PQ and BC. If this be reduced to 11°, the aperture will have its maximum value of 22°.

The aperture may be somewhat increased, and the length of the prism shortened, by using as the separating medium balsam of copaiba, as was suggested at the meeting of the Physical Society at which this paper was read.

The mean index of refraction for this substance is about 1·52, as determined by Brewster. The angle of total reflexion therefore for the ordinary ray is $\sin^{-1}(1{\cdot}52/1{\cdot}66)$, or about 66°, while for Canada balsam this angle is about 68°. The

possible aperture, using the balsam of copaiba, thus is about 24°.

Professor Thomson's prism, mentioned already, will have a wider field. But it must be remembered that the new prism was not designed for microscopic work, but to obviate the displacement in the image referred to at the commencement of the paper, and to produce a field in which the plane-polarization should be as nearly as possible complete.

PROCEEDINGS

OF

THE PHYSICAL SOCIETY

OF LONDON.

MAY 1883.

XXV. *Colour-Sensation.* *By* H. R. DROOP, *M.A.**

THE generally received theory of colour-sensation is that there are three colour-senses in the eye, and that all the different impressions of colour received by the brain are due to those three colour-senses being affected in different proportions by the light entering the eye. This theory was originally propounded by Young, and was revived by Helmholtz and Maxwell, who established experimentally certain laws of colour-sensation, from which laws the theory of three colour-sensations was a probable, but (as I shall show further on) not a necessary, inference.

Maxwell proved experimentally† that a linear equation of the form

$$X = vV + cC + uU$$

could always be found, expressing any colour and shade of colour perceived by the normally constituted eye in the terms of any three given colours (whether pigments—*e. g.* vermilion, chrome yellow, and ultramarine—or selected rays of the spectrum) as seen by the same eye. This equation, when the

* Read April 28, 1883.

† See Transactions of the Royal Society of Edinburgh, vol. xxi.; Philosophical Transactions, 1860, p. 57.

coefficients v, c, and u are all positive (*i. e.* when the three given colours are sufficiently intense and distinct from each other), means that the colour X could be produced by combining together (*i. e.* presenting to the eye simultaneously) certain proportions of the three given colours. From this it obviously followed that every such colour and shade *could* be produced by three colour-sensations, each of which, when excited, conveyed to the brain the impression of a homogeneous colour.

Helmholtz arrived at the same conclusion by proving (see *Handbuch der physiologischen Optik*, p. 282, ed. 1867) that any given colour could be produced by combining a certain quantity of white with some particular colour of the spectrum. From which he deduced that every colour and shade of colour depended on only three independent variables, viz. the quantity of the spectrum-colour, the quantity of white, and the length of wave of the spectrum-colour. But though the theory of three colour-sensations was the simplest and most obvious explanation of the experimental facts thus established, it is not (as has been commonly assumed) the only theory capable of explaining them. They would be equally well explained by supposing that there are four, five, or more colour-sensations connected by a sufficient number of linear equations of condition to reduce the number of independent variables to three. Obviously this will satisfy all that Helmholtz established. That it will also explain the law established by Maxwell may be shown as follows.

Suppose that there are four colour-sensations, R, Y, G, and B (the red, yellow, green, and blue sensations), and that each of them is expressed as a linear function of the three standard colours, V, C, and U, as every colour seen by a normal human eye can be expressed.

Then we shall have four equations of the form

$$\left.\begin{aligned} R &= v_r V + c_r C + u_r U, \\ Y &= v_y V + c_y C + u_y U, \\ G &= v_g V + c_g C + u_g U, \\ B &= v_b V + c_b C + u_b U. \end{aligned}\right\} \quad \ldots\ldots \quad (A)$$

And when we eliminate V, C, and U between these four equations we shall get a linear relation between R, Y, G, and B, which is the condition that these four colour-sensations

should be capable of being expressed as linear functions of V, C, and U, *i. e.* should be colour-sensations coexisting in a normal eye.

If we supposed five colour-sensations, we should have five equations (A) between which to eliminate V, C, and U, and should get two linear equations between the five colour-sensations.

The proposition that four colour-sensations with a linear relation between them will satisfy Maxwell's law may also be tested in another way, viz. by assuming that there are four colour-sensations connected by a linear equation

$$rR+yY+gG+bB=0, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

and that V, C, and U, the three standard colours, are known to result from these colour-sensations being effected in certain proportions.

E. g. V is known to result from R, the red colour-sensation, being effected to an extent r_v, Y to an extent y_v, G to an extent g_v, and B to an extent b_v; and may therefore be represented by the equation

$$V=r_vR+y_vY+g_vG+b_vB; \quad . \quad . \quad . \quad . \quad (2)$$

and similarly C and U may be represented by

$$C=r_cR+y_cY+g_cG+b_cB, \quad . \quad . \quad . \quad . \quad (3)$$

$$U=r_uR+y_uY+g_uG+b_uB. \quad . \quad . \quad . \quad . \quad (4)$$

Then from these four equations we can express R, Y, G, and B as linear functions of V, C, U. But every possible colour X must be produced by exciting all or some of the four colour-sensations, and therefore must be capable of being expressed by an equation

$$X=r_xR+y_xY+g_xG+b_xB.$$

Consequently, if R, Y, G, and B be replaced in this equation by the linear functions of V, C, and U which represent them, every such colour X can be expressed as a linear function of V, C, and U; *i. e.* every such colour will conform to the law, which Maxwell and Helmholtz established, of being capable of being made up of any three standard colours.

The same reasoning might obviously be applied in like manner to five colour-sensations connected by two linear equations of condition, or to n colour-sensations connected by $n-3$ linear equations of condition.

I have taken up this question and endeavoured to show that what Maxwell and Helmholtz established is not inconsistent with the existence of four or more colour-sensations (provided certain relations exist between them), because a certain recent discovery seems to me to have given a particular hypothesis, involving four colour-sensations, a strong claim to be accepted as, in the main, true. This discovery relates to the colours actually seen by colour-blind persons. Two persons have been discovered who, being each colour-blind of only one eye, can explain how far the colours seen by their colour-blind eyes agree with, or differ from, those seen by their normal eyes. It has been found that each of these persons has two colour-sensations complementary to each other. One sees yellow and blue, and is blind to red and green; while the other sees red and bluish green, and is blind to blue and yellow; and with each of them the combination of his two colour-sensations in proper proportions produces white or grey. Professor Holmgren, of Upsala, has given an account of both these cases in the 'Proceedings of the Royal Society,' vol. xxxi. p. 302; and Professor Hippel, of Giessen, has given an account (differing in some respects) of the first or blue-yellow case in Gräf's *Archiv für Ophthalmologie*, vol. xxvi. p. 176, vol. xxvii. pt. 3, p. 47.

These two cases suggested to Professor Preyer of Jena a theory (which he propounded in 1881 in Pflüger's *Archiv*, vol. xxv.) that ordinary eyes have two pairs of colour-sensations—(1) yellow and blue, and (2) red and bluish green, and that the colour-blindness which consists in confusing red and green, or, as the case may be, blue and yellow, is due to the absence of one pair of these sensations. But Professor Preyer does not deal with the difficulty that Helmholtz and Maxwell are supposed to have proved, that there cannot be more than three colour-sensations, although that view is treated as unquestionable by other recent writers, *e. g.* by Professor Donders (Gräf's *Archiv für Ophthalmologie*, vol. xxvii.), and by Professor v. Kries, of Freiburg im Breisgau (*Die Gesichtsempfindungen und ihre Analyse*, Leipzig 1882, p. 33); and it is naturally a serious obstacle to the fair consideration of Professor Preyer's theory.

But inasmuch as Professor Preyer supposes that in each pair of his colour-sensations the one sensation is complemen-

tary to the other, we have the equation

$$R + G = \text{White} = Y + B,$$

a linear relation between the four colour-sensations; and therefore it follows, from what I have already proved, that this hypothesis of two pairs of complementary colour-sensations is quite consistent with what Maxwell and Helmholtz established.

This theory of Professor Preyer's explains the leading facts of colour-blindness, viz. that a colour-blind eye only perceives two homogeneous colours, and that it is unable to distinguish between red and green, or, as the case may be, between blue and yellow. It is impossible to ascertain with absolute certainty that persons who are colour-blind with both eyes see the same colours as the two persons who have been discovered colour-blind of only one eye; but it is noteworthy that when Dr. Pole made that minute examination of his colour-blindness, the results of which he gave in the 'Philosophical Transactions' for 1859, he came to the conclusion that the colours he saw were yellow and blue and, as the result of their mixture, white; and he only gave up this view in deference to the three-sensation theory then supposed to be conclusively established.

The following facts, not connected with colour-blindness, seem to me to give considerable support to the hypothesis of two pairs of complementary colour-sensations:—

(1) Observations have been made as to the sensibility of different parts of the retina to different colours, and also as to the effect of diminishing the angles subtended at the eye by small coloured objects; and in both cases red and green colours are found to comport themselves alike, and differently from blue and yellow. When an object is viewed more and more indirectly, so that its image moves from the yellow spot towards the circumference of the retina, sensibility to yellow and blue lasts longer than sensibility to red and green; while, on the other hand, if the angular magnitude of the object be diminished, sensibility to red and green lasts longer than sensibility to blue and yellow (von Kries, *Gesichtsempfindungen*, pp. 93, 95).

(2) In cases where the colour-senses become affected by disease of the eye, the order in which different colours are

found to disappear agrees with the theory of four colour-sensations. In cases of atrophy of the optic nerve, it seems pretty clearly established that green becomes invisible first, then red, then yellow, while the perception of blue remains the longest (see Leber, *Archiv für Ophthalmologie*, vol. xv.; Leber, *Handbuch der Augenheilkunde*, vol. v. p. 1039; Schön, *Lehre vom Gesichtsfelde und seine Anomalien*).

In cases where the sight is affected by excess in alcohol or tobacco, Nuel found that green and red became invisible simultaneously, and blue and yellow later (*Annales de l'Oculiste*, 80, p. 110, as cited in von Kries, *Gesichtsempfindungen*, p. 156).

On the other hand, the received hypothesis of three colour-sensations does not readily explain how it is that one colour-blind eye sees blue, yellow, and white, and another red, green, and white, as Professors Holmgren and Hippel have found to be the case. If there are only three colour-sensations, it is generally agreed that they must be red, green, and violet. If one of these three sensations were wanting, we should naturally expect that the defective eye would have the other two sensations of a normal eye. For instance, if the red sensation were missing, we should expect that the eye would see green and violet, and white would appear of a bluish green complementary to the missing red. Similarly if green were missing, we should expect that the defective eye would see red and violet; while if violet were missing, it would see red and green, and white would appear yellowish complementary to violet.

The only attempted explanation* I have seen of this proceeds from Professor Donders, who suggests that where one of the three colour-sensations has been wanting from birth, its absence may have modified the development of the other two sensations. He says:—"The retina is not an instrument with three strings of which one has suddenly snapped. It is a living instrument whose three differently toned strings have been developed in conjunction with each other" (Gräf's *Archiv für Ophthalmologie*, vol. xxvii. p. 212).

* Professor Holmgren, when communicating the two cases of one-sided colour-blindness to the Royal Society, states that he considers these phenomena quite consistent with the theory of three colour-sensations; but he does not explain how he reconciles them with it, except by referring to works which I have not been able to get access to.

This explanation is ingenious; but it assumes that all cases of colour-blindness to red and green are from birth, whereas, though this is usually so, there are several alleged cases of acquired colour-blindness to red and green. Dr. Joy Jeffries, pp. 50–52, gives two cases, one discovered by Professor Tyndall, the other by Mr. Haynes Walton; and M. Nuel has also described one, *Annales de l'Oculiste*, 80, 82, as quoted by v. Kries, *Gesichtsempfindungen*, p. 154.

Moreover Professor Cohn (*Deutsche medicinische Wochenschrift*, 1880, No. 16, cited by von Kries, p. 158) claims to have temporarily restored to normal colour-vision a person affected with red-green colour-blindness from birth. If this be so, there cannot well have been any such abnormal development of his other colour-sensations as Professor Donders supposes. Such a development could hardly have been suddenly cured by artificial means.

Those colour-blind persons who cannot distinguish between red and green have been divided into two classes, according as they can perceive rays towards the end of the spectrum or are unable to do so. According to many adherents of the theory of three colour-sensations, those who can perceive rays at the red end want the green-colour sense, while those who cannot do so want the red sense. It seems to be established that rays at the green part of the spectrum do not make as much impression on the so-called green-blind as on the so-called red-blind; and Professor Donders has lately ascertained by careful measurements with two cases, one of so-called red-blindness, and the other of so-called green-blindness, that throughout the red and orange parts of the spectrum the red-blind eye perceives less light than the green-blind eye, and that the opposite is the case in the green portion of the spectrum (Gräf's *Archiv für Ophthalmologie*, vol. xxvii.; Transactions of the International Medical Congress for 1881, vol. i.p. 277).

But there seem to me to be several serious objections to explaining the difference between so-called green-blind and so-called red-blind by the hypothesis of three colour-sensations:—

(A) If there are only three colour-senses, the green sense must be of a yellowish green capable of producing yellow and orange when combined with the red sense, and very different from the bluish green which is complementary to red. There-

fore if one class of colour-blind persons have lost the red sense and the other the green sense, there ought to be considerable differences in all the colour equations obtained from the two classes of eyes, and especially in the proportions of blue and yellow which will neutralize each other. But I have not met with any trace of such differences having attracted attention, except in equations between red and green, and it is clear that in the spectrum the neutral point where the blue or violet colour-sense neutralizes the other colour-sense is very nearly the same for red-blind and green-blind persons. Professor Donders fixes it for his red-blind case at a wave-length of 494·85 millionths of a millimetre, and for his green-blind case at 502·3 millionths, the difference 7·5 being not one fiftieth part of the difference between the greatest and least wave-lengths in the visible spectrum; while Professor Preyer, in another case, found that doubling the amount of light altered the neutral point from 512·8 to 506·6, *i. e.* nearly as much (Pflüger's *Archiv*, vol. xxv.).

(B) If blindness to the red end of the spectrum were due to the absence of the red sense, it would be the same in extent in different red-blind persons, whereas in fact it differs considerably. (Donders, Gräf's *Archiv*, vol. xxvii.)

(C) The extent to which the violet end of the spectrum is obscured to violet- or blue-blind eyes also varies very much. Professor Stilling (*Klinische Monatsblätter für Augenheilkunde*, 1875, Beilage 2) met with three cases in which the green thallium-line between E and D formed the boundary of the visible spectrum; while in another case (*Centralblatt für praktische Augenheilkunde*, 1878, p. 99) the same observer found that nearly the whole of the spectral green was perceived, and grey or, in a faint spectrum, red beyond it. In the case of one-eyed violet- or blue-blindness described by Professor Holmgren ('Proceedings of the Royal Society,' vol. xxxii. p. 305) "the spectrum is continued over the place where we see green, greenish blue, cyan-blue, and indigo to the commencement of the violet, where it absolutely ended with a sharp limit about Fraunhofer's line G."

(D) As is well known, inability to distinguish between red and green has in many cases been found to exist among different members of the same family, and especially among

brothers; and therefore when such colour-blindness is found to exist among relations, there is a very strong probability that they have inherited the same affection of the eyes. Therefore if red-blindness and green-blindness be distinct things, due to the absence of different colour-senses, we should expec- that the colour-blind members of the same family would be either all red-blind or all green-blind. But this is not the case. Among the colour-blind cases examined by Professor Stilling (*Klinische Monatsblätter für Augenheilkunde*, 1875, Beilage 1 and 2) there were two pairs of brothers both unable to distinguish between red and green; and in each case one brother was able to perceive light at the red end of the spectrum, while the other was not.

All these reasons lead me to believe that the difference between so-called red-blind and so-called green-blind is not due to their having lost different colour-senses, but rather to the loss of one pair of colour-senses, those for red and green being, in the case of the so-called red-blind, accompanied by some disturbance of the other pair of colour-senses —a disturbance varying in character and degree in different cases, and similar to what is found to exist in different cases of blue- or violet-blindness.

The shapes of the curves Professor Donders has published (Trans. International Medical Congress, 1881, vol. i. p. 280) to represent the respective intensities of the light perceived by a red-blind and a green-blind eye have suggested to me a possible explanation of the difference between these two eyes. The curves representing the less-refrangible sensation of each eye correspond very nearly in shape and dimensions; but that for the red-blind eye is shifted some way further from the red end of the spectrum. On the other hand, the curves representing the more-refrangible sensation of the two eyes are almost identical in position as well as in shape and size. This effect would be produced if the organization producing the less-refrangible (or yellow) colour-sensation in the green-blind eye were so modified in the red-blind eye as to make shorter waves produce the same effects which longer waves produced in a green-blind eye. The change supposed would be equivalent to shifting the tone of a musical instrument an octave higher.

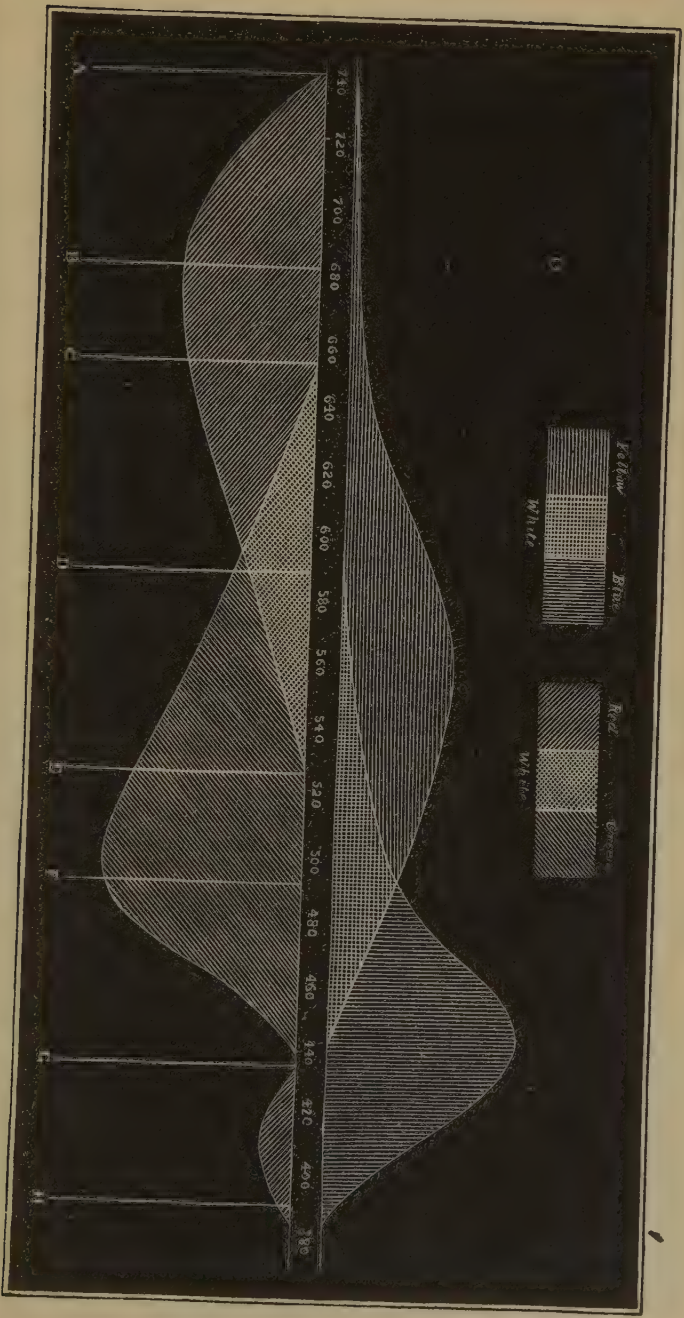
But I do not suppose that all cases of shortened spectrum could be thus accounted for.

In order to show more in detail what my views of colour-sensation are, I have prepared a diagram representing roughly how the two pairs of colour-senses are affected by the different rays of the spectrum. The upper half of the diagram represents the effects produced by the different rays on the yellow and blue colour-senses, and the lower part those they produce on the red and green colour-senses. The portion with crossed vertical and horizontal lines represents the extent to which the same rays operate on both the yellow and the blue colour-senses, and thus produce white light, which (combining with the colour produced by the colour-sense which is most affected) produces a whitish yellow or a whitish blue. Similarly the portion crossed by diagonal lines in the lower half of the diagram represents the extent to which the same rays operate on both the red and the green colour-senses.

I have represented the complementary colour-sensations as thus overlapping *, because Professors Preyer and Hippel have found that, for red-green blind eyes, the neutral point in the spectrum which appears white or grey varies in position according to the intensity of the light (Pflüger's *Archiv*, vol. xxv.; Gräf's *Archiv für Ophthalmologie*, vol. xxvii. pt. 3). Professor Preyer found that enlarging the aperture through which the light was admitted from ·250 millim. to ·370 millim. shifted the neutral point, where the spectrum appeared grey, from where the wave-length was 512·8 millionths of a millimetre to where it was 506·6 millionths. This is readily intelligible, if the rays in this part of the spectrum affect both the yellow and the blue colour-senses, while the intensity of the light alters the proportions in which they are respectively affected by it.

It will be observed that a narrow strip of yellow extends

* The extent to which the colour-sensations are represented as overlapping rests on conjecture. Observations on colour-blind persons, *i. e.* persons with only two colour-sensations, give readily the neutral points where the sensations counterbalance each other and produce white; but the extent to which they overlap could only be inferred from observing when the colours cease to have any admixture of white.


740
720
700
680
660
640
620
600
580
560
540
520
500
480
460
440
420
400
380
Yellow
White
Blue
Red
White
Green

nearly to the red end of the spectrum. Professor Holmgren ('Proceedings of the Royal Society') states that the red seen by the violet-blind eye, in his case of one-sided violet-blindness, is not quite identical with the common spectral red of the normal eye, but rather a clearer red having a shade of carmine, about the same as the red towards the end of the subjective spectrum of the normal-eyed. This colour would obviously require a slight admixture of yellow to reduce it to the common spectral red of the normal-eyed. Moreover the extension of the yellow nearly to the red end of the spectrum explains how it is that a great many persons who are red-green colour-blind can see almost to the red end of the spectrum.

The strip of red at the violet end of the spectrum also requires explanation. The colours seen by the red-green blind eye in the case of one-sided red-green blindness are not yellow and violet, but yellow and indigo with only a faint shade of violet in it. Indeed, while Professor Holmgren speaks to the faint shade of violet ('Proceedings of the Royal Society'), Professor Hippel, who discovered this case and had more opportunities of examining it, states that the blue lines of indium and cæsium (which are indigo, not violet) appeared the same to both eyes (Gräf's *Archiv*, vol. xxvii.). Therefore some addition is necessary to produce the deeper violet tints of the spectrum; and this can only be obtained by supposing that the violet rays affect the red colour-sense as well as the blue one.

This hypothesis, that violet results from combining the blue and red colour-sensations, is part of Professor Preyer's theory (see Sect. 38 of his paper in Pflüger's *Archiv*), and seems to me to be supported by several other facts.

(1) As I have already mentioned, when an object is viewed more and more indirectly, so that its image moves from the yellow spot towards the circumference of the retina, sensibility to yellow and blue lasts longer than the sensibility to red and green. On the other hand, if the angular magnitude of a coloured object be diminished, sensibility to red and green lasts longer than sensibility to blue and yellow. In each case violet behaves like a compound of blue and red. As the image moves towards the circumference of the retina, the violet object passes through blue into white, the red fading

first; while, as the angular magnitude of a violet object is diminished, it becomes reddish (v. Kries, *Gesichtsempfindungen*, p. 93).

(2) Again, Nuel (*Annales de l'Oculiste*, 80, 82, cited by v. Kries, p. 154) describes how in a certain case of acquired colour-blindness "violet appears blue, red and green white." Similarly Schön states that in cases of atrophy of the optic nerve, when green, red, and yellow are no longer recognized, blue alone is correctly designated, and violet is distinguished as dark blue (*Lehre vom Gesichtsfelde*, p. 23, cited by von Kries, p. 155).

(3) I have already mentioned a case of yellow-blue blindness described by Stilling, in which blue and violet were, in a faint spectrum, designated as red, though in a brighter spectrum they seem to have appeared grey.

All these facts seem to me to point to violet being the result of affecting at once the blue and red colour-senses.

I am moreover disposed to think that, in addition to the two pairs of complementary colour-senses, there is a fifth colour-sense for white.

Inasmuch as

$$R + G = Y + B = \text{White},$$

we have the two linear equations between five colour-sensations which are required to satisfy the laws which Maxwell and Helmholtz established. Therefore the hypothesis of a fifth, white, colour-sense is admissible.

That the eye does perceive white separately from any other colour is rendered at least probable by considering some particular cases in which this seems to occur.

(1) When an object is viewed indirectly, so that the image falls upon a part of the retina at a sufficient distance from the yellow spot, it will appear white or grey, whatever its actual colour may be (von Kries, pp. 91–95).

(2) If the angular dimensions of a coloured object be diminished, it will ultimately appear white or grey (von Kries, pp. 87, 94).

The more probable explanation in both these cases seems to be that the other colour-senses are no longer affected by the object, and only the white colour-sense remains affected by it.

(3) Every colour when intensely lighted up appears white (von Kries, p. 81). A not improbable explanation of this seems to be that the other colour-senses are only capable of being affected by light to a limited extent as compared with the white colour-sense.

(4) In cases of atrophy of the optic nerve the perceptions of different colours are gradually lost, until at length every colour appears grey (von Kries, p 154).

(5) There are also cases of total colour-blindness from birth, when every thing appears of the same colour with only different degrees of light and darkness. When this affects both eyes completely, it is of course impossible to predicate with absolute certainty what colour is perceived. But Becker (Gräf's *Archiv*, vol. xxv.) describes a case where only one eye was so affected, the other having normal vision; and I have seen another case described in which one half of each eye was completely colour-blind, the other half being normal. In each of these cases the colour-blind vision was of white. This white vision must have been arrived at either through the other colour-senses having been lost, leaving a white colour-sense behind, or through their having been modified into white.

XXVI. *On winding Electromagnets.* *By* Professors W. E. Ayrton, *F.R.S.*, *and* John Perry, *M.E.**

[Plates X. & XI.]

The following experiments were made to determine which mode of winding a given length of wire on an iron bar gave the strongest electromagnet for the same current. Four bars of iron, each 12 inches long, were cut from the same rod $\frac{3}{8}$ inch thick; and an exactly equal length of wire was wound on the four bars respectively, in the following way:—

1. Wire wound equally over the whole length (Pl. X. fig. 1).

2. Wire coned towards each end (fig. 2).

3. Wire wound equally over half the iron bar, leaving the other end bare (fig. 3).

* Read December 9, 1882.

4. Wire wound on one half but coned towards the end (fig. 4).

Electromagnet No. 1 was put so that its axis was at right angles to the axis of a small magnetic needle and passed through the point of suspension of the needle, which was suspended so as to move freely in a horizontal plane, and far enough away that the magnetic field due to the electromagnet No. 1, when magnetized by passing a current through it, was nearly constant over that portion of the field in which the little suspended needle moved when deflected. A constant current was now passed through the coil on No. 1, and the deflection of the little needle observed when No. 1 was placed at different distances from the centre of the test-needle, the axis of No. 1, however, always remaining in the same line. Under these circumstances it is well known that the strength of the field produced by No. 1 at the centre of the test-needle is approximately proportional to the tangent of its deflection. Experiments were now made in a similar way with electromagnet No. 2, and with each end of No. 3 and of No. 4, the same current as was employed with electromagnet No. 1 being used in all cases, and which was much below the saturating current.

The results obtained are given in the accompanying table, and are shown plotted in the accompanying curves (fig. 5), vertical distances representing the distance between the near end of the electromagnet and the centre of the test-needle, and horizontal distances the tangents of the deflection of the test-needle: A A A A is that for No. 1; B B B B for No. 2; C C C C for the covered end of No. 3; D D D D for the uncovered end of No. 3; E E E E for the covered end of No. 4; and F F F F for the uncovered end of No. 4.

Distance in inches between the near end of the bar and the centre of the test-needle.	No. 1.		No. 2.		No. 3.				No. 4.			
					Covered end.		Bare end.		Covered end.		Bare end.	
	Def.	Tan.	Def.	Tan.	Def.	Tan.	Def.	Tan.	Def.	Tan.	Def.	Tan.
3½	79°	5·14	77°	4·33	82°	7·12	57°	1·54	67°	2·30	27°	0·57
4	77	4·33	71	2·9	77	4·33	53	1·33	62	1·88	21	0·38
5	69½	2·67	58	1·6	66	2·24	46	1·04	52	1·28	14	0·28
6	59	1·66	47	1·07	56	1·48	39	0·81	43	0·93	11	0·19
7	50	1·19	37	0·76	46	1·04	32	0·62	36	0·73	9	0·16
8	42	0·9	30	0·58	37½	0·79	27½	0·52	29	0·56	7	0·12
9	35	0·7	24	0·46	30	0·58	18	0·32	22	0·48	4	0·07
10	30	0·58	20	0·36	·25	0·47	13	0·23	17	0·31	3	0·05

To ascertain the distribution of the lines of force, iron filings were sprinkled on paraffined paper, and the positions the filings took up fixed by the paraffin being softened by a heated piece of copper being passed over the paper at a short distance above it. These fields of force are shown in the diagrams 6, 7, 8, and 9 (Pl. XI.). From the curves in fig. 5 and from the iron-filing curves it is seen that the effect of coning the wire is to produce a strong field very near the pole, but that the force falls off very rapidly as the distance from the pole increases. With No. 2 magnet, for instance, the field between the poles is so weak that scarcely any definite arrangement of filings is traceable in the diagram 7 corresponding with it.

From the curves in fig. 5 it is seen that, at considerable distances from the end of the electromagnet, the uniformly coiled magnet No. 1 produces the most powerful field, while for points nearer the magnet, but still at a distance of about 3 inches from it, the covered end of No. 3 magnet, corresponding with the curve C C C, produces the strongest field, the next strongest being produced by the magnet No. 2 with the wire coned towards each end, since obviously the curve B B B cuts the curve A A A at a point corresponding with a distance of about 3 inches from the end of the magnet. For distances very close to the magnet, this method of experimenting cannot, of course, be employed to measure the resultant force accurately; and hence observations by this method at distances of less than 3½ inches from the end of the magnet to the centre of the oscillating needle were not made, and conclusions as to the resultant magnetic force very close to the poles must, of course, not be drawn from the curves in fig. 4.

Returning to the curves taken up by the iron filings, we see that No. 1 magnet gives an arrangement similar to that of an ordinary regularly magnetized bar-magnet. With No. 2 the lines around the poles are similar to those of No. 1, but the field between the poles is very weak. Magnets Nos. 3 and 4 give very similar figures, showing a very peculiar distribution of force. There is a great concentration of the lines at the pole corresponding to the half of the iron which is covered with wire; but the unwound end seems to form a

long weak pole, with its maximum force near the centre of the bar, *i. e.* at the inner end of the coil,—the differences between these two being, that with No. 4 magnet there is, comparatively, a greater concentration of force at the wound pole, and that the opposite pole is longer and extends a little way into the coil—the result of the coning of the wire. In these two cases the unwound end of the iron seems to act like an armature.

To ascertain the force which each magnet would exert on an armature, experiments were made and the following results obtained, the current flowing through the coil in each case being exactly the same, as well as the armature employed :—

Magnet.	Weight required to detach the armature from the covered end of the magnet.
No. 1	45 ounces.
2	57 „
3	57 „
4	77 „

These results confirm those previously obtained, that the field produced by the covered ends of the electromagnets numbers 2 or 3 at distances near the pole is much stronger than that produced by No. 1. But they show something else, viz. that for very small distances it is the covered end of No. 4 that produces the strongest field. In other words, returning to fig. 5, the curve *E E*, although much below the curves *A A*, *B B*, and *C C*, must rise rapidly and cut the others, just as the curve *C C* cuts the curve *A A*, at a point corresponding with a distance of about 4·2 inches from the end of the magnet, and just as, again, the curve *B B* cuts *A A* at a point corresponding with a distance of about 3·2 inches from the end of the magnet. The curves of iron-filings (fig. 9) indeed give indication of the great strength and concentration of field there is produced close to the iron by the wire coned at the end, as employed in the magnet No. 4.

With, then, a definite iron core, a definite length of wire to be coiled on it, and to be traversed with a definite current, the mode of coiling to produce the largest field depends entirely on the distance from the end of the electromagnet at which the field is to be produced. With the particular magnet we

have employed we see that, at distances from the end of the magnet very small compared with the length of the core, the wire should all be coiled up at the near end of the core, as in fig. 4; to create a field at a distance from the end of the magnet equal to about a third of the length of the magnet, it is better to coil the wire uniformly over one half of the core, as in fig. 2, than to cone it up at the near end as in 4; while for distances from the end of the magnet equal to, or greater than, about $\frac{4}{9}$ of the length of the core, the uniform mode of winding is the best.

We have to thank two of our students, Messrs. Sayers and Pink, for most cordial assistance rendered us in this investigation.

XXVII. *Experiments on the Viscosity of a Solution of Saponine. By* W. H. Stables *and* A. E. Wilson, *Yorkshire College, Leeds* *.

M. Plateau has shown (*Statique des Liquides*, t. 2, ch. vii.) that a body placed in a liquid and wetted on one side only, experiences in many cases a greater resistance to its motion than if it were completely immersed. Some controversy has arisen between Marangoni and himself as to the cause of this phenomenon, which M. Plateau explains by the assumption that the liquids in question possess a surface viscosity greater than that of the interior. The liquid in which the surface resistance is in most striking contrast to that of the interior is a solution of saponine in water.

Oberbeck (Wiedemann's *Annalen*, Bd. 11, S. 634) has repeated and extended Plateau's experiments, using an oscillating disk instead of a magnetic needle. He made no observations upon saponine solution. The object, therefore, of the following investigation is to study the movements of a disk when oscillating in or near the surface of a solution of this substance.

Oberbeck found that the resistance of a water-surface increased largely with exposure to the air; but he also proved that even with fresh distilled water the resistance is considerable. As he points out, we are therefore led to one of

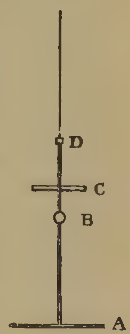
* Read April 14, 1883.

two conclusions—namely, that either water has a surface viscosity different from that of the interior, or else that a pure water surface cannot be obtained.

The apparatus used in our experiments was similar to that employed by Grotrian (Pogg. *Ann.* Bd. 157, S. 237).

It is fully described by him; and it is therefore unnecessary to figure here the connexions of the different parts. The diagrammatic representation in fig. I. may, however, conduce to clearness.

Fig. I.



It consists of a circular disk A of nickel-plated brass 76·25 millim. in diameter and 2 millim. thick. In the centre is screwed a brass rod, to which a concave mirror B and a small iron bar C (used to set the apparatus in motion by means of a magnet) are attached. A wire (119·8 centim. long), employed to put the apparatus in motion by torsion, is firmly gripped between two small plates at D.

The brass rod by which the plate was suspended could be unscrewed and replaced by others of different sizes.

The plate was suspended in the centre of a circular glass dish 15·5 centim. in diameter and 8 centim. deep. This was fitted with a wooden cover, through which a thermometer was introduced. The whole was surrounded by a case having glass sides, which served to ward off air-currents. The time of the oscillations was measured by a stop-watch indicating quarter-seconds.

Three rods were used in turn to suspend the plate. They were of the same length, but of different radii.

The radius of (1) was ·1337 centim.
„ (2) „ ·2360 „
„ (3) „ ·3362 „

The scale on which the beam of light was reflected was about 2·5 metres from the mirror; the maximum half-amplitude was about 200 millim.; so that the half-oscillation was about 2°·3.

The moment of inertia of the apparatus was determined by means of a brass ring of the same external diameter as the plate, and was found to be 753·09 (C.G.S.).

The plate was first carefully levelled, and distilled water poured into the vessel to within a few millimetres of the bottom of the plate. Water was then added with a pipette until the bottom of the plate and the surface of the water touched.

By a simple calculation the difference in height caused by the addition of a measured quantity of water was determined. An addition of 2 cubic centim. of water was found to cause a rise of ·1 millim. in the level.

The elevation was of course increased when the disk was in the surface; but the change is allowed for in the calculations.

The temperatures of the air and of the water were carefully noted at the commencement of each observation, and were found to remain nearly constant. To attain this end the experiments were conducted in a cellar, and special precautions were taken to prevent any marked variations in temperature. The variations therefore did not exceed 1° C.; and the small corrections thus rendered necessary were made by means of the table of the values of the coefficient of viscosity of water at different temperatures given by Grotrian (Pogg. *Ann.* Bd. 157, S. 242). All the observations were thus reduced to 16° as a standard temperature. The time of oscillation was ascertained by observing the time of 10 swings. This operation was repeated a number of times and the mean taken. The logarithmic decrement was calculated from the readings obtained from the graduated scale.

The plate was suspended by each of the three rods in turn immersed to a depth exceeding 1 centim., and the logarithmic decrement and mean time of oscillation determined for each. The following table shows the results obtained with the saponine solution and with water:—

TABLE I.

	Water.		Saponine.	
Rod.	Time of oscillation.	Log. dec.	Time of oscillation.	Log. dec.
	sec.		sec.	
1...........	5·19	·0477	5·24	·0785
2...........	5·20	·0479	5·26	·1424
3...........	5·22	·0483	5·26	·2045

The great difference between the surface-properties of saponine and those of water is here made very evident. The alterations in the dimensions of the rod which produced a slight effect only in the case of water increased the logarithmic decrement in the case of saponine two and a half times, a result which could only have been due to the increase of the section in contact with the surface.

If we assume that the disk oscillated under the influence of two forces, one of which (that of torsion) is proportional to the angular displacement from the position of rest, while the other, due to the viscosity of the liquid, is proportional to the velocity, the latter is measured by $M\lambda/T$, where M is the moment of inertia, λ the logarithmic decrement, and T the time of an oscillation. If, as in the case of a saponine solution, the surface resistance be so great that the friction between the surface layer and the interior may be neglected with regard to it, $M\lambda/T$ would be approximately of the form $a+br^2$, where r is the radius of the rod and a and b are constants.

The values of these, determined from the above equations by the method of least squares, are $a=8{\cdot}42$, $b=191{\cdot}6$.

Using these coefficients to calculate the value of $M\lambda/T$ from the observations on the saponine solution, we obtain the following results:—

TABLE II.

r.	$M\lambda/T$.	
	Observed.	Calculated.
0		8·42
·1337	11·27	11·85
·2360	20·37	19·07
·3362	29·36	30·07

The numbers are perhaps in as good agreement as could be expected, if we remember that the theory on which they are calculated is only approximate. If in the case of water we neglect b, the value of a is 6·92.

After the above preliminary observations a careful series of

experiments was made in which the plate was gradually immersed to a greater depth in water.

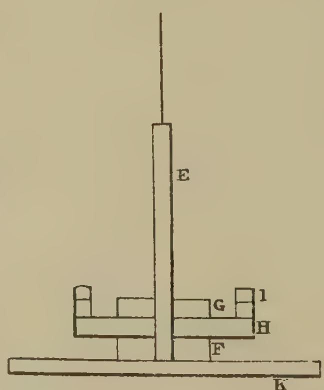
The following are the results obtained :—

Position of plate.	Time of oscillation.	Log. decrement.
	sec.	
Upper edge of plate ·7 mm. *above* level of water	5·06	·0258
·56 millim. above	5·10	·0259
·28 " "	5·10	·0260
·14 " "	5·11	·0262
Top of plate level with surface of water	5·10	·0266
·1 below	5·12	·0272
·2 "	5·11	·0272
·3 "	5·13	·0285
·4 "	5·17	·0298
·5 "	5·18	·0329
·7 "	5·20	·0366
·9 "	5·20	·0415
1·3 "	5·20	·0479
1·7 "	5·20	·0488

In the above results, the level of the liquid is corrected for the displacement of the liquid by the plate.

Similar experiments were next made on the saponine solution ; but it was at once seen that the apparatus used in the previous experiment with the water was quite unsuitable on account of the great resistance offered by the surface. Certain modifications were therefore introduced. In this second form (fig. II.) a steel piano-wire was used, the length of which was 98·35 centim. and the diameter 1·025 millim. Suspended by this wire was a stout brass rod, E, terminating in a disk, F, upon which rested a similar movable disk, G ; and between the two disks could be fitted a brass plate, H, carrying heavy brass rings, I. To the bottom of the lower disk an (unmagnetized) steel bar K was attached, by means of which the apparatus was set in oscillation by the use of a

Fig. II.



magnet. The apparatus used in the water experiment was attached to the centre of this bar by a strip of metal attached to D (fig. I.) and soldered to the bar. Great care was taken to render the junction perfectly firm, so that no torsion could possibly take place at this point. The moment of inertia was determined as before, and found to be 186653 (C.G.S.).

The first fact which was evident from the experiments was that, although the apparatus when suspended in water oscillated isochronously, it did not do so when suspended in the saponine solution. The following table gives the results of a number of experiments on the time of two long and two short swings respectively in that solution:—

Large amplitude.	Small amplitude.
10·90 sec.	10·28 sec.
10·70 „	10·28 „
10·75 „	10·35 „
10·52 „	10·35 „

With a thicker rod:—

10·42 sec.	9·85 sec.
10·52 „	9·73 „
10·33 „	9·90 „

Similar experiments in the case of water gave for two long oscillations, 10·25; for two short, 10·30; for three long oscillations, 15·5 and 15·75; and for three short, 15·6 and 15·75 seconds.

The amplitudes denoted long and short are not all of equal size; and as those called "large" are much larger than those actually used in the experiments, the correction which might otherwise have been necessary has been neglected.

The following are the results of the experiments on saponine solution:—

Position of plate. Upper surface	Time of oscillation.	Log. decrement.
·14 millim. above liquid...	9·51 sec.	·1960
level........................	9·50 „	·2520
·1 below surface	9·55 „	·0067
·2 „	9·60 „	·0045
·3 „	9·63 „	·0039
·4 „	9·58 „	·0034
·6 „	9·55 „	·0030
1·0 „	9·62 „	·0025
1·4 „	9·63 „	·0022
2·4 „	9·62 „	·0020
3·65 „	9·59 „	·0019

These results are shown on the accompanying curve (p. 241).

With regard to the first two observations, in which the plate was oscillating in the surface of the liquid, only two complete oscillations were obtainable for each determination; and as the logarithmic decrement was found to diminish considerably as the amplitude increased, a number of observations at different amplitudes were taken. These were plotted down in the form of a curve, showing the amplitudes and logarithmic decrement; and from these curves the logarithmic decrement for an arbitrary standard initial amplitude of 500 divisions was taken. The slope of these curves was so considerable that our observations can only be considered as giving an inferior limit to the resistance of the surface of the saponine solution. When the plate was once immersed below the surface, it was found that twenty or more oscillations were readily obtained, and that the magnitude of the original amplitude had little or no effect. The variations of temperature were small (the difference being only 0°·7 C.); and as their effect on the surface-viscosity is unknown, no correction was made for them. The error thus introduced would, however, as the regularity of the curve shows, be small.

These observations, then, enable us to compare the resistance offered to a disk when oscilating in, or just below, the surface of a saponine solution and of water.

Thus we get for the surface of saponine,

$$\frac{M\lambda}{T} = \frac{186653 \times \cdot 252}{9 \cdot 5} = 4951;$$

and for the surface of water,

$$\frac{M\lambda}{T} = \frac{753 \times \cdot 0266}{5 \cdot 1} = 3 \cdot 927.$$

At ·1 millim. below the surface these numbers change to 131 and 4 respectively. At the surface, therefore, the ratio of the resistances is 1261; and at ·1 millim. below it is 33; while in the interior it is, as has been shown, $\frac{8 \cdot 42}{6 \cdot 93}$, or 1·2. Although, therefore, these numbers can only be taken as approximations to the truth, we think that they enable us to make an estimate of the magnitude of the resistance offered to a body

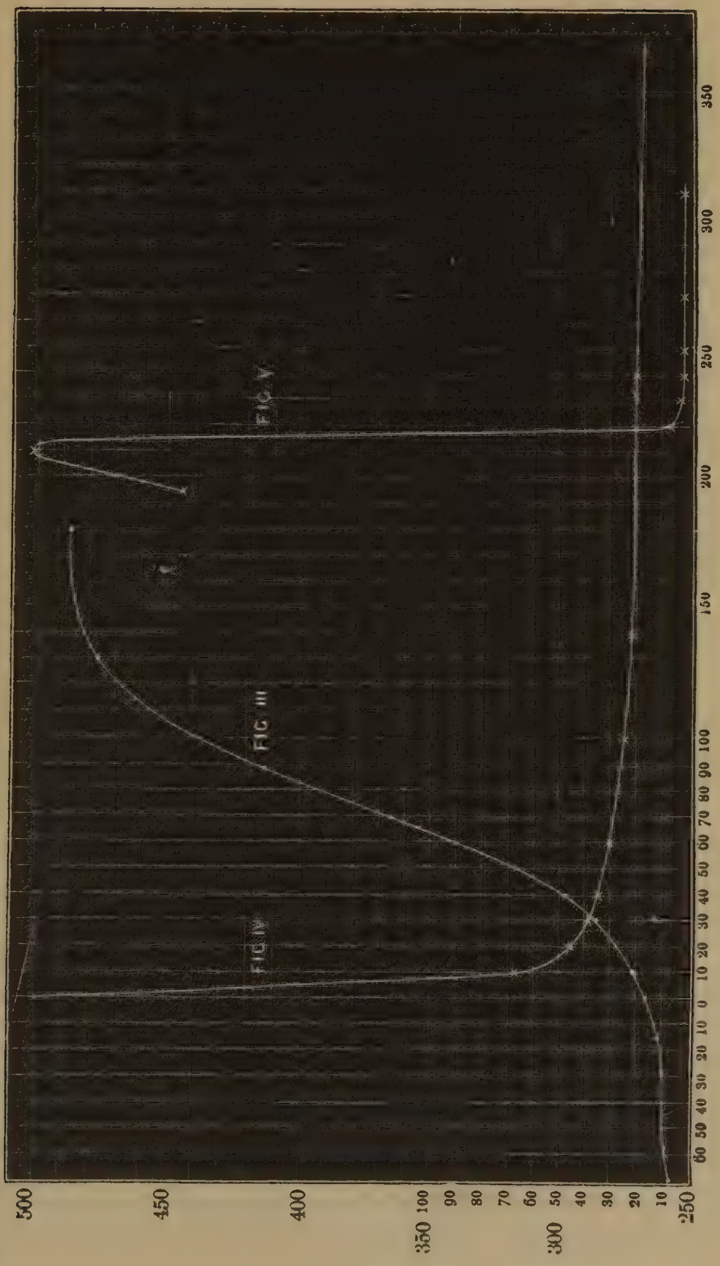


FIG. III
FIG. IV
FIG. V
500
450
400
350
300
250
100 90 80 70 60 50 40 30 20 10
60 50 40 30 20 10 0 10 20 30 40 50 60 70 80 90 100
150
200
250
300
350

oscillating in the surface of saponine solution, for which no previous experiments afforded the required data.

They show that whereas the resistance offered to an oscillating disk, 2 millim. thick, in the surface of water is only about half what it is in the interior, at the surface of a 2-per-cent. saponine solution it is at least 600 times greater than in the interior, but that this ratio is reduced to 16 by immersing the upper surface of the disk to a depth of 0·1 millim.

Special experiments proved that the logarithmic decrement in air was so small that the resistance of the air might safely be neglected when the comparisons of the various resistances were made as above described.

Explanation of the Curves.

Fig. III. is the curve given by the logarithmic decrements obtained from the experiments on water. The abscissæ are expressed in terms of hundredths of a millimetre; they represent the distance of the upper edge of the plate from the surface, and are negative when it is above it. The ordinates represent the logarithmic decrement in terms of ·0001, the lowest horizontal line corresponding to the value of ·0250.

The gradual increase in the value of the logarithmic decrement as the plate is more deeply immersed is clearly shown.

Fig. IV. refers to the observations made on the saponine solution. In this case the values of the ordinates are to be taken from the small figures. The positions corresponding to the numbers obtained when the disk was in the surface cannot be shown on the scale of the diagram. Fig. V. therefore has been drawn on one tenth of the scale of fig. IV. To avoid confusion it has been displaced to a convenient distance along the line of abscissæ. The enormous increase of resistance as soon as the disk touches the surface is very strikingly shown; and it must be remembered that the increase for a very small oscillation would be very much greater.

The conclusion may be drawn from figs. III. and IV., that both in water and in the saponine solution the effect of the surface disappears when the edge of the disk is about a millimetre and a half below it.

We cannot conclude without expressing our sincere thanks to Prof. Rücker for his kind assistance in our experiments.

XXVIII. *On Curved Diffraction-gratings.* *By* R. T. Glazebrook, *M.A., F.R.S., Fellow and Lecturer of Trinity College, Demonstrator at the Cavendish Laboratory, Cambridge**.

Prof. Rowland has described the appearances presented when a beam of light, after passing through a slit, falls on a grating ruled on a cylindrical surface, and has given a very elegant construction for determining the position of the diffracted foci in the case in which the principal section of the grating is a circle and the source of light is placed at its centre of curvature. The mathematics of the subject have been dealt with still more recently by M. Mascart (*Journal de Physique*, January 1883) and Mr. W. Baily (Phil. Mag. March 1883). The object of the present paper is to carry the discussion somewhat further.

Prof. Rowland claims for his gratings that they enable him to form a pure spectrum without the use of lenses, and hence have an immense advantage over those hitherto employed. It must, however, be remembered that the formulæ obtained to give the position of the diffracted spectra are only true to a first approximation, that the spectra formed and the source of light are to one another in the relation of the conjugate geometrical foci of a lens or mirror. All the waves which arrive at any one point of the spectrum are not in exactly the same phase. Aberration effects are produced, and have to be considered just as in the ordinary theory of lenses or mirrors. Now, if a plane wave of light fall on a plane grating, and the effects be observed on a screen at an infinite distance behind the grating, the spectrum formed is perfectly pure; all the red light, after passing the grating, is definitely brought to a focus at one point; there is no aberration, so far at least as the grating is concerned. Of course the difficulty is to obtain the plane wave and the screen at an infinite distance. If the source of light be placed at the principal focus of a collimating lens, the emergent wave differs from a plane by quantities depending on the aberration of the lens; while if the diffracted beam is received on a second lens and a screen be placed in the focal plane of that lens, the screen would

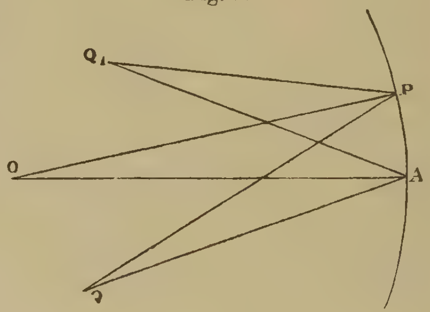
* Read April 14, 1883.

practically be at an infinite distance from the grating but for the aberration produced by the lens.

So far, then, as definition merely is concerned, we have to compare the aberration effects produced by these lenses with those caused by the curvature of the grating. Of course a reflexion grating used without lenses has an immense advantage for experiments on the violet or ultra-violet rays which are absorbed by glass.

In considering the aberration, then, we shall follow the method adopted by Lord Rayleigh in his paper on "Investigations in Optics, with special reference to the Spectroscope. Aberration of Lenses and Prisms" (Phil. Mag. January 1880).

Fig. 1.



Let QA, QP be two adjacent rays diverging from a point Q and falling on the concave side of a circle AP, centre O. Let $QAO=\phi$, $AOP=\omega$, $QA=u$, $OA=a$. Then

$$QAP=\phi+\frac{\pi}{2}-\frac{\omega}{2},$$

$$AP=2a\sin\frac{\omega}{2}.$$

Hence

$$QP^2=u^2+4a^2\sin^2\frac{\omega}{2}-4au\sin\frac{\omega}{2}\sin\left(\frac{\omega}{2}-\phi\right); \quad . \quad . \quad (1)$$

and, expanding as far as ω^3, we find

$$QP=u+a\omega\sin\phi-\frac{a\omega^2}{2}\left(\cos\phi-\frac{a}{u}\cos^2\phi\right)$$
$$-\frac{a\omega^3\sin\phi}{2}\left(\frac{1}{3}-\frac{a}{u}\cos\phi+\frac{a^2}{u^2}\cos^2\phi\right)+\ldots \quad (2)$$

Again, let Q_1 be another point on the other side of the normal OA, and let $Q_1A=u'$, $Q_1AO=\psi$. Then

$$Q_1P=u'-a\omega\sin\psi-\frac{a\omega^2}{2}\left(\cos\psi-\frac{a}{u'}\cos^2\psi\right)$$
$$+\frac{a\omega^3\sin\psi}{2}\left(\frac{1}{3}-\frac{a}{u'}\cos\psi+\frac{a^2}{u'^2}\cos^2\psi\right). \quad . \quad . \quad (3)$$

Suppose now that A is a point on one line of the grating, and P a corresponding point on some other line. Then waves from Q diffracted at A and P respectively will reach Q_1 in the same phase if $QP+Q_1P=QA+Q_1A\pm n\lambda$, λ being the wave-length. That is, if

$$\begin{aligned} &a\omega(\sin\phi-\sin\psi) \\ &\quad-\frac{a\omega^2}{2}\left\{\cos\phi+\cos\psi-a\left(\frac{\cos^2\phi}{u}+\frac{\cos^2\psi}{u'}\right)\right\} \\ &\quad-\frac{a\omega^3}{2}\left\{\sin\phi\left(\frac{1}{3}-\frac{a}{u}\cos\phi+\frac{a^2}{u^2}\cos^2\phi\right)\right. \\ &\quad\left.-\sin\psi\left(\frac{2}{3}-\frac{a}{u'}\cos\psi+\frac{a^2}{u'^2}\cos^2\psi\right)\right\}=\pm n\lambda. \qquad (4) \end{aligned}$$

This is equivalent to Mr. Baily's formula carried to the next degree of approximation; and his results are obtained by neglecting the term in ω^3 and taking ϕ and ψ to satisfy the equation

$$\sin\phi-\sin\psi=\pm\frac{n\lambda}{a\omega}, \quad . \quad . \quad . \quad . \quad . \qquad (5)$$

and then u and u' to satisfy

$$\cos\phi+\cos\psi-a\left(\frac{\cos^2\phi}{u}+\frac{\cos^2\psi}{u'}\right)=0. \quad . \quad . \qquad (6)$$

To consider the aberration we have two cases before us. Let us suppose (1) that equation (5) holds, and determine the value u'_1, say of u', considering the terms in ω^3 in equation (4). This will give us what we may call the longitudinal aberration.

In the second case we shall suppose equation (6) to hold, and determine the value for ψ which satisfies (4) to the same approximation. This will give us the lateral aberration.

In the general case equation (4), as it stands, really determines the locus of the image of Q formed by diffraction at the two lines A and P; and this locus is clearly an hyperbola, with A and P as foci. Waves diffracted at A and P respectively will arrive in the same phase at any point of this hyperbola. For every point such as P on the grating an hyperbola possessing similar properties can be drawn. If all these hyperbolas meet in a point, then that point is really a focus for waves diverging from Q; they all are in the same phase when they meet there. This is the case if the grating be plane and Q and Q_1 infinitely distant. If, however, the hyperbolas do not all meet in a point, there is really no focus in its strict sense, only a geometrical focus. If we neglect ω^3 and higher

terms, then the point given by (5) and (6) is to this approximation common to all the hyperbolas: it is the geometrical focus.

To discuss, then, the aberration in this case. Let $u'_1 = u' + \delta u'$, where u' satisfies (6), and suppose we neglect $\overline{\delta u'}|^2$, $a\omega\delta u'$, and such terms. Then

$$\cos\phi + \cos\psi - a\left\{\frac{\cos^2\phi}{u} + \frac{\cos^2\psi}{u'}\left(1 - \frac{\delta u'}{u'}\right)\right\}$$
$$+ a\omega\left\{\pm\frac{n\lambda}{3a\omega} - \frac{a\sin\phi\cos\phi}{u}\left(1 - \frac{a}{u}\cos\phi\right)\right.$$
$$\left. + \frac{a\sin\psi\cos\psi}{u'}\left(1 - \frac{a}{u'}\cos\psi\right)\right\} = 0.$$

Thus

$$\delta u' = -\frac{u'^2\omega}{a\cos^2\psi}\left\{\pm\frac{n\lambda}{3a\omega} - \frac{a}{u}\sin\phi\cos\phi\left(1 - \frac{a}{u}\cos\phi\right)\right.$$
$$\left. + \frac{a}{u'}\sin\psi\cos\psi\left(1 - \frac{a}{u'}\cos\phi\right)\right\}. \quad . \quad . \quad (7)$$

Equation (7) determines the aberration in the general case. To determine the effect of this in practice, let us suppose that we are considering the spectrum of the first order, so that the retardation of the light coming from two consecutive lines is just one wave-length; and hence, if P be on the kth line from A, and σ the distance between two lines, then the arc AP $= k\sigma = a\omega$, and $n = k$.

Let us suppose, further, that the origin of light is at the centre of curvature of the grating, so that $u = a$, $\phi = 0$, and hence, taking the $-$ve sign in (6),

$$\sin\psi = \frac{\lambda}{\sigma}, \cos\psi = \sqrt{\left(1 - \frac{\lambda^2}{\sigma^2}\right)}, u' = a\cos\psi = a\sqrt{\left(1 - \frac{\lambda^2}{\sigma^2}\right)}.$$

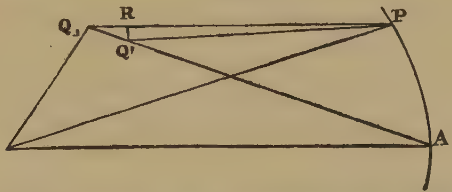
Thus

$$\delta u' = + a\omega\frac{k\lambda}{3a\omega} = +\tfrac{1}{3}k\lambda. \quad . \quad . \quad (8)$$

Let Q_1 (fig. 2) be the point on the line given by $\sin\psi = \frac{\lambda}{\sigma}$, which is determined by $u' = a\cos\psi$, Q' being the point on that line at which light arrives in exactly the same phase from A and P. Then $Q_1Q' = \frac{1}{3}k\lambda$. And the angle PQ_1A differs by only a small quantity from ω; while, since $Q'Q_1$ is small compared with PQ', the angle Q_1PQ' is small compared with ω.

Hence, if $Q'R$ be drawn at right angles to PQ_1, the light from P arrives at R in the same phase as at Q', and the difference

Fig. 2.



in phase at Q_1 between the waves coming from A and P is $Q_1Q' - Q_1R = Q_1Q'(1 - \cos\omega)$; and to the same approximation this is equal to $\frac{1}{6}k\lambda\omega^2$. So that if we consider as well the light coming from a point k' lines below A, the extreme difference of phase in the various waves which reach the point Q_1 is $\frac{1}{6}(k+k')\lambda\omega^2$; $k+k'$ will be the total number of lines in the grating.

Thus in one of Prof. Rowland's gratings we have

$$a = 213 \text{ centim.},$$

$$\omega = \frac{37}{2130} \text{ about},$$

$$k + k' = 14250;$$

and hence the difference in phase is about $7\lambda/10$. Hence the aperture of the grating is too large to give the best definition: for that purpose the difference of phase in the various secondary waves arriving at the point in question should not be greater than $\lambda/4$.

We may conveniently express this difference of phase in terms of the number of lines, the radius of the grating, and the distance between the lines. Let σ be the distance between the lines; then

$$\omega = \frac{(k+k')}{2a}\sigma,$$

and the difference of phase is

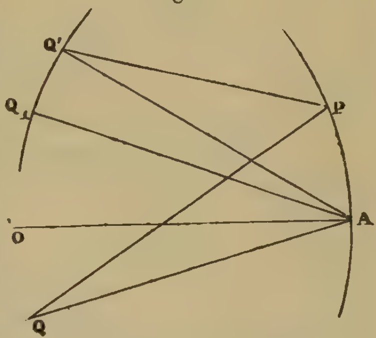
$$\frac{1}{24} \times (k+k')^3 \frac{\sigma^2}{a^2} \lambda.$$

For good definition this difference of phase must not be greater than $\lambda/4$. Since in the case above the difference of phase is $7\lambda/10$, we must reduce the number of lines, keeping the distance between them the same, in the ratio of $\sqrt[3]{10}$ to $\sqrt[3]{28}$, or

rather more than 2 to 3. Hence by covering up rather less than one third of the grating we should expect to produce better definition.

In another grating of Rowland's, $\sigma=\frac{1}{11400}$ centim., $a=520$ centim., $k+k'=160{,}000$; and in this case the difference of phase comes out to be about $4\cdot8\times\lambda$. The grating is much too wide; it will require reducing in the ratio of $1:\sqrt[3]{19\cdot2}$, or about $3:8$.

Fig. 3.



To consider now the lateral aberration, using the same notation, describe a circle (fig. 3) through Q_1, with A as centre. Light from A arrives in the same phase at all points on this circle. Let Q' be the point on the circle at which the light arriving from P is in the same phase as that from A, and let $\psi+\delta\psi$ be the angle OAQ', and let ψ, ϕ, u, and u' satisfy (5) and (6). Our fundamental equation (4) becomes, neglecting terms like $\omega^2\delta\psi$,

$$\begin{aligned}a\omega\Big[&\sin\phi-\sin\psi-\delta\psi\cos\psi\\&-\frac{\omega}{2}\left\{\cos\phi+\cos\psi-a\left(\frac{\cos^2\phi}{u}+\frac{\cos^2\psi}{u'}\right)\right.\\&\left.-\sin\psi\,\delta\psi\left(1-\frac{2a\cos\psi}{u'}\right)\right\}\\&-\frac{\omega^2}{2}\left\{\sin\phi\left(\tfrac{1}{3}-\frac{a}{u}\cos\phi+\frac{a^2}{u^2}\cos^2\phi\right)\right.\\&\left.-\sin\psi\left(\tfrac{1}{3}-\frac{a}{u'}\cos\psi+\frac{a^2}{u'^2}\cos^2\psi\right)\right\}\Big]=\pm n\lambda. \quad (9)\end{aligned}$$

Hence

$$\begin{aligned}\delta\psi&\left\{\cos\psi-\frac{\omega}{2}\sin\psi\left(1-\frac{2a\cos\psi}{u'}\right)\right\}\\&+\frac{\omega^2}{2}\left[\sin\phi\left\{\tfrac{1}{3}-\frac{a}{u}\cos\phi\left(1-\frac{a}{u}\cos\phi\right)\right\}\right.\\&\left.-\sin\psi\left\{\tfrac{1}{3}-\frac{a}{u'}\cos\psi\left(1-\frac{a}{u'}\cos\psi\right)\right\}\right]=0\,;\end{aligned}$$

and, to the approximation adopted in considering the longitudinal effect,

$$\delta\psi = -\frac{\omega^2}{2}\sec\psi\left\{\pm\tfrac{1}{3}\frac{n\lambda}{a\omega} - \frac{a}{u}\sin\phi\cos\phi\left(1-\frac{a\cos\phi}{u}\right)\right.$$
$$\left.+\frac{a}{u'}\sin\psi\cos\psi\left(1-\frac{a}{u'}\cos\psi\right)\right\}. \quad . \quad . \quad (10)$$

If, as before, Q coincide with O, then $u=0$, $\phi=0$, $u'=a\cos\psi$; and taking the negative sign, so that $\sin\psi=\frac{n\lambda}{a\omega}$,

$$\delta\psi=\frac{\omega\sec\psi\times n\lambda}{6a}, \quad . \quad . \quad . \quad . \quad . \quad (11)$$

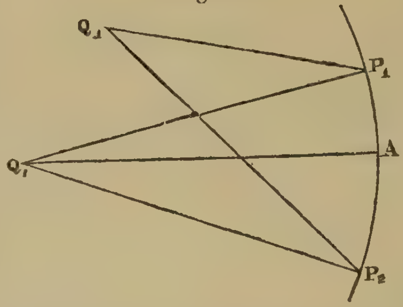
and $$Q_1Q'=u'\delta\psi=\frac{n\lambda\,\omega}{6}.$$

Thus, carrying our approximation as far as terms in ω^3 in equation (4), we find that the position of the image formed, considering only two of the lines as producing diffraction effects, is not at Q_1 but Q', where $Q_1Q'=\frac{n\lambda\omega}{6}$. Hence, if we consider the whole grating, using the same notation as before, the breadth in a direction normal to AQ_1 of the image formed will be comparable with $\frac{(k+k')\lambda\omega}{6}$, ω being the whole semi-aperture. Expressing this in terms of the radius of the grating a and the distance between the lines σ, we find the value $\frac{1}{6}(k+k')^2\frac{\lambda\sigma}{a}$. Thus the breadth of the image will depend on the square of the number of lines. In the grating first considered this quantity, $\frac{1}{6}(k+k')\lambda\omega$, is about $\frac{1}{500}$ of a centimetre for yellow light, while the distance between the D lines is about $\frac{1}{50}$ centim., or ten times as much; while in the second grating this lateral aberration is $\frac{1}{50}$ centim., the distance between the D lines being about seven times as great. If the size of this last grating be reduced to $\frac{3}{8}$ of what it actually is, the extreme lateral aberration will be reduced to $\frac{9}{64}$ or about $\frac{1}{7}$ of its actual value, thus becoming about $\frac{1}{350}$ of a centimetre, and the extreme difference of phase in the light of a given wavelength λ reaching any point of the diffracted spectrum will never exceed $\lambda/4$, the dispersion will remain unaltered, the definition and the brightness of the spectrum will both be increased.

It is clear that in both cases the outer portion of the grating

not merely impairs the definition, but actually renders it less bright than before. For consider two points P_1, P_2 (fig. 4) equidistant from A, such that the difference in phase in the waves coming from P_1 and P_2 to Q_1 is $\frac{\lambda}{2}$ (since the difference of phase for the extreme rays is in both cases greater than $\frac{\lambda}{2}$, these points can be found). Then the light reaching Q_1 from above P_1 is opposite in phase to some of that which reaches Q_1 from between P_2 and A, and tends to neutralize the effect of this; while similar results hold for light coming respectively from below P_2 and between A and P_1. Thus a large aperture does not necessarily mean that there is a large quantity of light at the focus. Exactly the same may happen in the case of a lens. Lord Rayleigh has shown that if α be the angular semi-aperture of the lens as viewed from the focus, and the curvatures of the lens be adjusted to reduce the longitudinal aberration to a minimum, α^4 should not exceed λ/f. A similar course of reasoning shows us that if α^4 is greater than $2\lambda/f$, the light from the outer annulus of the lens will be opposite in phase to that from the central portions.

Fig. 4.



To compare, finally, the definition of the curved grating with that produced by a plane grating, and two lenses of equal focal length used as a collimator, and the object-glass of a telescope respectively, we can show (Parkinson, 'Optics,' § 130), that if α is the semi angular aperture of either of these lenses seen from its principal focus, f its focal length, and the curvatures are adjusted to make the aberration of each lens a minimum, then the aberration is, for light of refractive index 1·5, $\frac{15}{7} f\alpha^2$; but, as quoted above, Lord Rayleigh has shown that the aberration should not be greater than λ/α^4. Hence α^4 must not be greater than $\frac{7\lambda}{15f}$.

In the case of the first of Prof. Rowland's gratings discussed above, the slit and eyepiece are at a distance of about 200 centim. from the grating. Let us suppose we are using two lenses of 200 centim. focal length, and inquire what their aperture may be to allow the condition above given to be satisfied. If y be the radius of the lens, we have

$$y^4 \text{ not } > \text{ than } \frac{7 \times 8 \times 10^6 \times 6}{15 \times 10^5}.$$

Thus y must not be greater than 3·8 centim. A lens of this aperture would just about admit the light from the whole of the actual grating 5 centim. × 7 centim. in area if it were plane; whereas, without the lens, to obtain the best definition we are restricted to the use of about two thirds of the grating.

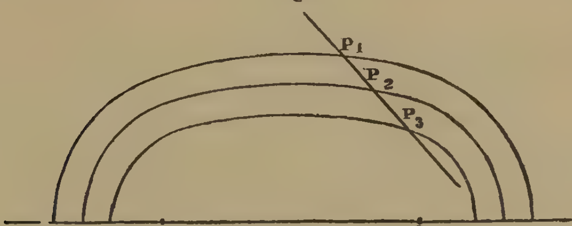
In the case of the other grating, we may, without increasing the size of the apparatus, use a lens of 500 centim. focal length; for good definition its aperture should not be more than

$$\sqrt[4]{\frac{7 \times 125 \times 10^6 \times 6}{15 \times 10^5}},$$

or about 7·6 centim. A lens of this aperture would enable us to use with the best advantage the whole of the grating if it were plane; whereas in the concave grating, for good definition we should only use about three eighths of the whole. It would seem, then, that in cases in which there is no objection to the use of glass (because of its absorbing qualities), a large grating may be used to greater advantage if it be ruled on a flat surface and properly chosen lenses be employed with it, than if the grating be curved.

It may be instructive to consider the subject briefly in another manner. Let Q, Q_1 (fig. 5) be any two points, and

Fig. 5.



with Q and Q_1 as foci describe a series of confocal ellipses; let the major axes of these ellipses increase in arithmetical progres-

sion, and let the common difference be λ. Consider a spherical wave diverging from Q and reflected at any point of any one of these ellipses; all the reflected light will reach Q_1 in the same phase. Take any surface P_1, P_2, &c. cutting the ellipses in P_1, P_2 &c., and suppose it capable of reflecting light at these points and incapable of so doing elsewhere. All the light from Q which falls on this surface at these points will be reflected to Q_1, and the various waves will reach Q_1 in the same phase. If now Q be the section of a slit normal to the paper, $P_1 P_2$ &c. that of a polished cylindrical surface whose generators are normal to the paper, and lines be ruled on this surface to block out the spaces $P_1 P_2$, $P_2 P_3$, &c., the lines also being normal to the paper, we shall obtain a diffraction-grating which will give an image of Q without aberration at Q_1.

We can thus determine the law according to which lines must be ruled on any cylindrical surface to give an aplanatic diffraction-image of a slit; for we require only to write down the equations to the ellipses and the surface and determine the points of intersection. We will solve the simple case when the curve $P_1 P_2$ &c. is a straight line parallel to $Q Q_1$. Take $Q Q_1$ as axis of x; let a and b be the semi-axes of one of the ellipses, suppose that which touches the line $P_1 P_2 \ldots$; then the semi major axes of the other ellipses are

$$a+\frac{\lambda}{2}, \quad a+\delta \ldots a+\frac{n\lambda}{2}, \text{ \&c.};$$

while to find b_n, the semi minor axis of the $(n+1)$th ellipse, we have

$$\left(a+\frac{n\lambda}{2}\right)^2-b_n^2=a^2-b^2 . \mathrm{A} b_n^2=b^2+na\lambda+\frac{n^2\lambda^2}{4}. \quad . \quad (12)$$

Let $Q Q_1=2c$, then we have

$$a^2=b^2+c^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

and the equation to the $(n+1)$th ellipse is

$$\frac{x^2}{a^2+na\lambda+\frac{n^2\lambda^2}{4}}+\frac{y^2}{6^2+na\lambda+\frac{n^2\lambda^2}{4}}=1. \quad . \quad . \quad (14)$$

Let x_n be the abscissa of the point in which this is cut by the line $y=b$, then

$$x_n^2=\frac{\left(a^2+na\lambda+\frac{n^2\lambda^2}{4}\right)\left(na\lambda+\frac{n^2\lambda^2}{4}\right)}{b^2+na\lambda+\frac{n^2\lambda^2}{4}}. \quad . \quad . \quad (15)$$

Substituting for a and giving n the values 0, 1, 2, 3, &c. in order, we can obtain values for x_0, x_1, x_2 &c., and determine thus the position of the lines. A plane grating ruled in this manner would form at Q_1 without aberration an image of Q for light of the given wave-length λ. Of course it would be open to the objection which holds against all such aplanatic arrangements, viz. that they are only good for light of one definite wave-length. If the grating were used for light of a different refrangibility, the image formed would suffer from aberration.

XXIX. *A new Photometer.*

By Sir JOHN CONROY, *Bart.*, *M.A.**

HAVING recently made a considerable number of photometric observations, and learnt by experience the difficulty which attends all such determinations, I venture to bring before the Society the description of a new form of photometer which appears to possess certain advantages over those in use. All such instruments, with the exception of the wedge-photometer, are essentially arrangements for comparing the illuminating-power of two lights, and therefore do not give absolute measures; the one I propose describing is no exception to this general rule.

I had intended to use, in some experiments on the amount of light reflected by metallic surfaces, the ordinary Bunsen's disk; but I found that, owing to the small size of the beam of reflected light, it was impossible to make any satisfactory measurements with the disks in common use, and after trying various photometric arrangements I finally adopted a modification of Ritchie's photometer.

The various forms of shadow-photometers work well; but as the accuracy of the determination depends on the edge of the two shadows coinciding and yet not overlapping, it is necessary to have some arrangement for altering the distance between the screen and the shadow-producer, which adds to the complexity of the apparatus, except indeed when, as in Mr. Harcourt's photometer for gas-work, the variation in the relative intensities of the two lights is caused by the size of one of the flames being altered, and not, as in those arrange-

* Read April 28, 1883.

ments heretofore in use, by altering the distance of the flame from the screen whilst the size is kept constant.

Ritchie's photometer, as is well known, consists of two pieces of white paper fastened to the adjacent sides of a triangular block of wood, each being illuminated by one only of the lights which are to be compared. Finding it impossible so to arrange the apparatus that the illuminated surfaces should be actually in contact, the bend in the paper along the edge of the block separating the two illuminated areas, and therefore interfering with the accuracy of the determination, I placed one of the pieces of paper slightly in front of the other, and overlapping it to a small extent, so that, whilst both were visible to the observer, each was illuminated by one only of the sources of light; when equally illuminated, the edge of the front paper vanished.

It was originally intended that the light should be incident upon the surfaces of the paper at an angle of 45°; but it was found that when the light regularly reflected by the paper reached the observer (*i. e.* when the line of sight and the direction of the incident light formed equal angles with the normal to the paper) it was not possible to make satisfactory measurements.

After various positions had been tried, it was found that the best results were obtained when the light was incident upon the paper at an angle of about 30° and the line of sight formed an angle of 60° with the normal.

Two triangular blocks of wood, 4 centim. high, were screwed to a rectangular board about 15 centim. by 10 centim., in the position shown in the figure, and pieces of white paper, 3 centim. by 3 centim. (filter-paper was tried; but ordinary white writing-paper not too highly glazed seemed most suitable), held against the hypotenuse of each of the triangular prisms by india-rubber bands.

$\frac{1}{4}$ actual size.

It is of course essential that the light should be incident upon both papers at equal angles, and that the papers should be so placed that no light can

be reflected from one to the other. It is desirable that both papers should be cut from the same sheet, and that the surfaces on which the light is incident should originally have formed one side of that sheet.

A rectangular board, similar to that to which the prisms were fixed, was fastened to the top of the prisms by two screws; and to the edges of this board four strips of card, in three of which square apertures had been cut, were fixed, and the whole arrangement painted both externally and internally a dead black.

In order to adjust the papers, or replace them by new ones, it is merely necessary to withdraw the two screws in the top board and lift it off, together with the sides of the box.

The edge of the front paper coinciding with the middle line of the box, the photometer could be used with either side uppermost; and in order to be certain that the illumination of both papers was entirely due to light incident directly upon them, measurements of the relative intensity of two similar paraffin-lamps were made with the photometer in both positions; and it was found that the readings were identical.

The photometer was compared with a Bunsen's disk by placing it at the end of a horizontal board furnished with a scale, and along which a paraffin-lamp was arranged to slide. A Bunsen's disk, in an ordinary form of support with two inclined looking-glasses, could be screwed to the end of the board, to which three stops were so fixed that, when the disk was removed and the new photometer placed against the stops, the middle line of the box was in the same vertical plane as the disk had been.

A paraffin-lamp was placed on either side of the photometer, the position of one remaining constant, whilst that of the other was altered until the illumination was equal, and the distance of the latter read off, in centimetres, on the scale.

The table gives the results of eight observations made with both photometers, the differences of each observation from the mean, and also the squares of these differences.

Bunsen's Disk.

	centim.		Differences from the mean.	Squares of the differences.
	85·7	....	+ ·6	·36
	84·6	....	− ·5	·25
	84·9	....	− ·2	·04
	85·4	....	+ ·3	·09
	86·2	....	+1·1	1·21
	84·8	....	− ·3	·09
	84·3	....	− ·8	·64
	85·2	....	+ ·1	·01
Mean . .	85·1		Sum . .	2·69

New Photometer.

	85·0	....	−·4	·16
	85·7	....	+·3	·09
	85·0	....	−·4	·16
	85·3	....	−·1	·01
	85·2	....	−·2	·04
	85·4	....	−·0	·0
	85·8	....	+·4	·16
	85·7	....	+·3	·09
Mean . .	85·4		Sum . .	0·71

The probable error of the mean result and the probable error of a single observation were found by the ordinary formulæ, $0{\cdot}6745\sqrt{\frac{\text{sum of the squares of the differences}}{n(n-1)}}$ and $\sqrt{n} \times$ the probable error of the mean result, n being the number of observations.

	Bunsen's disk. centim.	New photometer. centim.
Probable error of mean result	$\pm 0{\cdot}148$	$\pm 0{\cdot}076$
,, ,, single observation	$\pm 0{\cdot}418$	$\pm 0{\cdot}215$

The new photometer therefore appears to be twice as accurate as the Bunsen's disk: it is only fair to add that, had the measurements been made by an observer accustomed to work with the disk, the result might have been different.

XXX. *On the Determination of Chemical Affinity in terms of Electromotive Force.*—Part VII. *By* C. R. Alder Wright, *D.Sc. (Lond.), F.R.S., Lecturer on Chemistry and Physics, and* C. Thompson, *Demonstrator of Chemistry, in St. Mary's Hospital Medical School**.

On the Electromotive Force of Clark's Mercurous-Sulphate Cell; and on the Work done during Electrolysis.

On the E.M.F. of Clark's Cell.

133. In the course of the series of experiments partly described in Parts V. and VI. a large number of observations have been made with various cells after Clark's construction (Proc. Roy. Soc. xx. p. 444), in all cases compared with one another and with other cells by means of the quadrant-electrometer only, so that they never generated any current other than the minute leakage current through the not mathematically absolutely insulating materials between their poles.

In some instances the mercurous sulphate was purchased (from Messrs. Hopkin and Williams), and was well washed before use by numerous boilings with distilled water and decantations. In other cases the mercurous sulphate was prepared by heating twice-distilled mercury (previously purified by nitric acid) with pure sulphuric acid at as low a temperature as possible consistent with any action taking place, and thoroughly washing the resulting sulphate by repeatedly boiling with distilled water and decantation. The action was never allowed to go on until more than a fraction of the mercury used was converted into sulphate, in order to reduce the amount of mercuric sulphate formed to a minimum.

The cells were made out of pieces of ordinary combustion-tubing (selected on account of the absence of lead in the glass) drawn out before the blowpipe into the U-shape represented on about two thirds scale in the cut (fig. 1). The glass being

* Read May 12, 1883.

perfectly dry and hot, pure recently-boiled still hot mercury was poured into them so as to form a layer about half an inch (10 to 15 millimetres) deep, *a*; on the top of this was then poured a boiling paste of thoroughly well-washed mercurous-sulphate and zinc-sulphate solution, containing so much of the latter salt as to be slightly supersaturated when cold, so as to crystallize on standing. It was found convenient to make the paste not too thick, and to let the solid matter subside in the cell, the supernatant comparatively clear fluid being sucked out by a clean pipette, so as finally to leave on the top of the mercury a layer of particles of mercurous sulphate wetted with zinc-sulphate solution some 15 to 20 millimetres deep, *b*. Pieces of zinc rod (cast in glass tubes from pure metal fused in a porcelain crucible), well brightened by a file that had never touched any other metal, were then placed in the cells so as to dip into the paste some 4 or 5 millimetres, and project out of it about twice as much, *c*. The zincs were kept from falling by pieces of cork, *d*, cut as represented in fig. 2, and previously immersed in hot paraffin-wax so as to expel air and moisture; to the ends projecting from the paste were previously soldered copper wires, *e*. Melted paraffin-wax was then poured into the cell so that all air was expelled, rising through the perforations in the edges of the cork disks, and so that the upper two thirds of the zinc and the soldering were completely covered, *f*. Finally, a piece of platinum wire, *g*, or a strip of foil was passed down the narrow limb of the cell so as to make contact with the mercury: it was found convenient to amalgamate the tip of the platinum by moistening it and immersing it in freshly made sodium amalgam, all sodium being removed from the adherent film of mercury by subsequent immersion in water for some hours. The cells thus prepared, being wanted for use only and not being required to be externally well finished, were not mounted in the neat

Fig. 1.

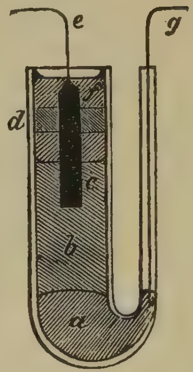


Fig. 2.

brass cases with ebonite tops and binding-screws usually employed, but were simply fixed in a beaker, or any other convenient holder, by pouring in melted paraffin-wax around them. When used in connexion with the electrometer, the copper wire soldered to the zinc and a similar wire soldered to the strip of platinum (and secured by a turn round the upper end of the narrow limb and a drop of sealing-wax) were bent over so as to dip into mercury-cups, a number of which were arranged in the arc of a circle round two others, like those figured in Part V.; so that any consecutive pair of cups could at will be connected with the electrometer by the double switch.

Either through a natural repulsion between bright zinc and the mercurous-sulphate paste, or through the formation of a faint film of grease &c. on the zinc from the file used to brighten it, it sometimes happened that the cell when finished would not work, contact not existing between the zinc and paste. It was found that this never occurred when the brightened zincs were washed successively with ether, alcohol, and saturated zinc-sulphate solution just before immersion in the paste.

134. On comparing together a moderately large number of cells (upwards of fifty) thus prepared with different specimens of mercurous sulphate, readings being taken two or three times a week for some three months, the following results were obtained:—A slight rise in E.M.F. was often observed during the first few days after construction; but at the end of a week at most the values *became constant, and remained so (the temperature being constant) for long periods of time.* The maximum variations observed between the average results of the series of observations for any two given cells were slightly less than that found to exist by Clark (whose highest and lowest values are respectively 1·4651 and 1·4517 volt, giving a difference of ·0134 volt, or upwards of 0·9 per cent.) Taking the average of the whole set as 100·00, the maximum variation between two single cells did not exceed ·010 volt, or 0·7 per cent., each cell possessing a value lying between 99·65 and 100·35. Even amongst cells set up at the same time from absolutely the same materials, extreme differences of as much as 0·005 volt = 0·35 per cent. were sometimes observed,

although usually the difference did not exceed ·002 or ·003 volt and was frequently almost inappreciable.

Much greater differences, however, were found to exist when the zinc-sulphate solution was not completely saturated with that salt, the variation produced being of this kind, that *the weaker the solution the higher the E.M.F. of the cell*, the difference being approximately proportionate to the amount of dilution, and amounting to upwards of 2·0 per cent. of the value when considerably dilute zinc-sulphate solution was used. The details of these observations will be discussed in a future paper, along with those of similar experiments made with other cells. It may, however, be here noticed that, so long as a cell containing unsaturated zinc-sulphate solution was protected against concentration by evaporation, and was only used in connexion with a quadrant-electrometer, its indications remained perfectly constant for many months (the temperature being the same), precisely as was found with cells set up with saturated zinc-sulphate solution.

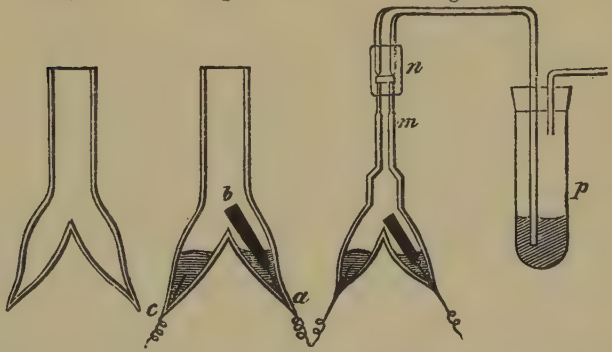
Effect of Dissolved Air on the E.M.F. of Clark's Cell.

135. Two series of experiments were made with the object of finding out how far the boiling of the mercurous-sulphate paste (as recommended by Clark) in order to remove dissolved air is essential. In one series a number of cells were set up, using fully aerated zinc-sulphate solution and unboiled mercury (exposed to the air under a glass shade for several days since preparation and distillation respectively); in the other the paste was boiled in a Sprengel vacuum produced in the cell itself for some time, the cell being then hermetically sealed, so as to reduce the amount of residual air to a minimum. In each case the average E.M.F. of the combination *was sensibly identical with that of an average ordinary Clark cell* prepared as above described and containing zinc-sulphate solution of the same strength as that contained in the combination.

In order to prepare these hermetically-sealed cells a rather troublesome process was employed. First a piece of glass tubing, about 10 or 12 millim. in bore, was blown into a Y-shape, and the two limbs of the Y drawn out as represented in fig. 3; a zinc rod was then cast so that a thin platinum wire was imbedded in one end ; this zinc rod was brightened and

sealed up on the tube so that the platinum wire projected (fig. 4, *a*). By the aid of a glass funnel with a flexible capil-

Fig. 3. Fig. 4. Fig. 5.



lary stem (made by drawing out a piece of tubing before the blowpipe) paraffin-wax was introduced into the sealed-up limb so as to cover up completely the platinum wire and lower half of the zinc, leaving the other half exposed, *b*. In a similar way, recently boiled mercury was run into the other limb, previously sealed up with a second platinum wire passing through the glass, *c*. The stem of the inverted Y-tube was then carefully drawn out before the blowpipe (fig. 5, *m*), and connected by means of a short piece of india-rubber tubing, *n*, with the end of a glass tube projecting from the little flask, *p*, containing mercurous sulphate paste, and connected with the Sprengel pump. When a fairly good vacuum was obtained, the paste was made to boil by applying a very gentle heat; after about half an hour's boiling (the pump being at work the whole time) the connexion between the pump and flask was suddenly severed, when the sudden access of atmospheric pressure drove the paste into the cell, completely filling it; the pump was then again connected, and the boiling carried out again in the cell itself, and so on as before. Finally, by means of a blowpipe the drawn-out stem was sealed at *m*. To prevent the paste blocking up this drawn-out part, it was found necessary to use levigated particles of mercurous sulphate with a large proportion of zinc-sulphate solution; so that ultimately the cell contained much more fluid than solid matter. In order to use the cell, copper wires were soldered to the plati-

num wires projecting from the sealed ends of the inverted Y and bent over so as to dip into mercury-cups, the Y being either held by a clamp or imbedded in paraffin-wax, and of course being never allowed to be upset or shaken up so that the mercury could pass into the limb containing the zinc, which is otherwise liable to occur and spoil the cell.

136. The following numbers may be quoted as illustrations of the practical absence of any effect on the E.M.F. of the cell caused by the presence or otherwise of dissolved air. The values cited are the average readings, during a period of several months, of a dozen cells set up with cold-saturated zinc-sulphate solution well aerated, and *not* sealed up with melted paraffin-wax, but only loosely corked to avoid entrance of dust. Each cell during this period remained sensibly constant. All the values are reduced to the average reading (taken as 100·00), during the same period, of a yet larger number of cells prepared hot and sealed up precisely in accordance with Clark's directions—this average reading being the standard employed in the previous portions of these experiments, and especially in Parts V. and VI.

Batch of four cells made from mercurous sulphate purchased from Messrs. Hopkin and Williams	No. 1.	99·90
	„ 2.	99·94
	„ 3.	99·95
	„ 4.	99·99
Batch of four cells made from mercurous sulphate prepared specially by ourselves for this purpose	„ 5.	99·75
	„ 6.	99·95
	„ 7.	99·99
	„ 8.	100·02
Batch of four cells made from another specimen of mercurous sulphate prepared by ourselves	„ 9.	99·97
	„ 10.	100·11
	„ 11.	100·18
	„ 12.	100·19
General average		99·995

Precisely analogous figures were obtained with several vacuum-prepared cells, no one of which gave a value outside of the limits 99·7 and 100·3, *i. e.* outside of the limits of fluctuation of the ordinary Clark's cells compared with them. On opening one of these vacuum-cells so as to admit air, a distinct fall in E.M.F., amounting to 0·25 volt, was observed ;

this behaviour, however, was not shown by other similar cells on opening.

Influence of Mercuric Sulphate in the Mercurous Sulphate.

137. However carefully the mercurous sulphate may be prepared, it is almost impossible to obtain it without some admixture of mercuric sulphate. During the boiling and washing by decantation this latter becomes a basic salt, the so-called "turpeth mineral," which possesses a bright yellow tint, and communicates to the mercurous sulphate a more or less pronounced yellowish tinge. In order to see how far the presence of varying quantities of this compound might possibly affect the E.M.F. of Clark's cell, several cells were set up in the same way as the hot-prepared cells described above, but using turpeth mineral only instead of mercurous sulphate. Two samples of turpeth mineral were employed:—one purchased (from Messrs. Hopkin and Williams), and well washed by boiling up many times with water and decantation before use; the other prepared by boiling mercury with a large excess of pure sulphuric acid, evaporating off most of the acid (which process converts all mercurous sulphate present into mercuric), adding to a large bulk of boiling water and washing many times the yellow heavy powder formed, by boiling up with water and decanting, so as to remove all traces of free sulphuric acid, and of the soluble acid mercuric sulphate also formed. On taking a long series of readings of these cells, it was found that whilst the E.M.F. was, when the cell was newly set up, close to that of an average mercurous-sulphate cell, on standing a few days a distinct fall was observable, which went on progressively until, after some weeks, a diminution in the E.M.F. of between 3 and 4 per cent. was brought about, after which the fall ceased or became very languid. Thus the following average readings were obtained as before, the average E.M.F. of the hot-prepared cells containing saturated zinc-sulphate solution being taken as 100 when at the same temperature as the cells examined: cells A, B, C, and D were set up simultaneously with turpeth mineral prepared by ourselves; cells E and F simultaneously with the purchased substance. The zinc, zinc sulphate, and mercury used were the same as those used for the hot-prepared

cells. Notwithstanding, however, that all the cells were as alike as possible, yet the rate of fall during the first few weeks was by no means identical.

Age of cell...	1 day.	2 to 6 days.	1 to 2 weeks.	6 weeks.	2 to 4 months.	6 to 20 months.
Cell A	100·6	100·12	99·95	98·35	97·04	97·00
" B	100·3	100·31	100·11	99·65	98·18	97·64
" C	100·4	100·22	99·90	99·19	97·38	96·96
" D	100·6	100·46	99·80	97·88	97·39	97·44
Average	100·5	100·28	99·93	98·77	97·50	97·26
Cell E	99·4	98·85	97·27	......	96·78	95·80
" F	99·5	99·41	98·13	......	97·11	96·00
Average	99·45	99·13	97·70	......	96·95	95·90

It is evident from these figures that the effect of the presence of turpeth mineral in the mercurous sulphate used for Clark's cells is in the direction of decreasing the value; but inasmuch as the decrease is progressive, whilst no such alteration was observed in the Clark's cells examined, at any rate during several months after construction, it appears doubtful whether the variations in the E.M.F. of different Clark's cells set up at various times can be attributed to this cause.

Permanence of Clark's Cells.

138. A number of cells prepared in various ways (paste boiled and cells sealed with paraffin-wax; paste boiled *in vacuo* and cells hermetically sealed; set up with saturated zinc-sulphate solution, or with weaker solutions) were kept for periods of time ranging from a few months to two or three years, and checked against one another from time to time, or compared with Daniell cells set up as described in Part V., with amalgamated pure zinc and electro-copper plates, and pure zinc- and copper-sulphate solutions of the same molecular strength*.

* A large number of observations on the E.M.F. of Daniell cells have shown that, when proper precautions are taken in setting up the cells, a very considerable degree of constancy in value is attainable, so that such cells serve as good practical standards; but that if these precautions are neglected, *variations amounting to 5 per cent., and even more, may ensue.* The essential precautions are:—first, that pure solutions of zinc and copper sulphates containing no free acid should be used, each being of the same

No permanent changes in the values were observed (outside of the limit of the errors of observation) in the case of those cells which were so well sealed that neither evaporation took place, nor passage outwards of the fluid by capillary action through cracks in the sealing material. Vacuum-cells were thus kept unchanged for upwards of two years, as also were some normal Clark cells that were completely imbedded in paraffin-wax. In several cases, however, where the cells were not completely imbedded, but were only sealed up by a plug of paraffin-wax poured in at first round the zinc plate and the cork &c. supporting it, cracks formed sooner or later either in the paraffin-wax itself or between the glass and the wax, so that the fluid passed out through the cracks by capillary action and formed an efflorescence outside the cell. In some cases the action went on to such an extent as to leave the zinc wholly exposed, no contact finally existing between it and the paste: such cells were of course utterly spoilt. In other instances the zinc was only partially bared: in these cases the E.M.F. of the cell remained almost unaltered when saturated zinc-sulphate solution was employed in the first instance, but was lessened when unsaturated solution was originally used, owing to the evaporation and concentration which went on simultaneously with the capillary action, or subsequently to the commencement thereof. For example, two cells set up with zinc-sulphate solution about two thirds saturated gave the values 101·07 and 100·92 during the first few weeks

molecular strength (*i. e.* practically of the same specific gravity; conveniently the molecular strength may be near to MSO_4, $50\,H_2O$); secondly, that the solutions should be in separate vessels, united when required by an inverted U-tube, the mouths of which are covered with thin bladder (Raoult's form of cell); thirdly, that the plates should be pure zinc amalgamated with pure mercury, and copper recently electro-deposited from pure sulphate solution—the wires serving as electrodes, and their junctions with the plates being coated with gutta-percha, so that no part of the plate or wire is simultaneously in contact with both fluid and atmosphere; and fourthly, that, if used to generate a current, the current-density must not exceed some 5 microampères per square centimetre, so that with plates exposing 20 square centimetres the total resistance in circuit must be *at least* 10,000 ohms, if exposing 10 square centimetres 20,000 ohms, and so on.

after construction; cracks then formed, and efflorescence and evaporation took place, so that the zincs became partially bared, during which time the electromotive forces gradually sank. After some months the paste became covered with crystals of zinc sulphate, indicating that the residual solution moistening the mercurous sulphate was saturated: the electromotive forces were then 100·13 and 99·73 respectively, which values were subsequently retained almost constant for several months longer, notwithstanding that a considerable portion of each zinc rod was out of the paste and exposed to the air.

A number of observations made with cells containing zinc rods partly immersed in the paste and partly exposed to the air, gave sensibly the same average result as another series of observations made with the same cells when the zinc rods were pushed down so as to be wholly immersed (the upper end and the wire serving as electrode being protected from contact with the paste by gutta-percha).

It is specially to be noticed, in connexion with the question of the permanence of Clark's cells, that the cells experimented with were only used in connexion with the quadrant-electrometer; so that from first to last they *never generated any continuous current, nor had any current (however small) sent through in the inverse direction*—conditions impossible completely to realize in practice when working by the "method of opposition" or with the potentiometer.

Effect of Temperature on the E.M.F. of Clark's Cell.

139. According to Clark (Proc. Roy. Soc. xx. p. 444), the E.M.F. of a hot-prepared mercurous zinc-sulphate cell diminishes at an approximately constant rate of 0·06 per cent. per degree rise in temperature between 15°·5 and 100°; he states, however, that this figure might be verified with advantage. A number of observations having indicated, as a preliminary result, that this value is considerably too high between the temperature-range (10° to 25°) most frequently obtaining in practice, and that fairly constant results are given with different cells, the following experiments were made in order to determine more exactly the mean coefficient of alteration per

degree between these temperature-limits, with the result of showing that, instead of Clark's number (0·0006) being deduced, a value but little above two thirds of this figure was obtained, viz. ·000411, as the average of ten experiments with five cells.

Let the E.M.F. of a given cell, taken temporarily as a standard, be 1 at temperature t_1 (near to 15°), and let the E.M.F. of a second cell compared therewith be a_1 when the cell compared is at a temperature t_2, the standard being still at t_1. In another experiment, when the standard is at a temperature t_3 not far from t_1, let the E.M.F. of the second cell be a_2, this cell being at the temperature t_4. Now let x be the mean coefficient of variation for 1° between t_2 and t_4 for the second cell, whilst x' is the analogous coefficient between t_1 and t_3 for the temporary standard. Then, since the E.M.F. of the standard at t_1 is unity, its E.M.F. at t_3 is $1-(t_3-t_1)x'$, whence the E.M.F. of the second cell at t_4 is $a_2\{1-(t_3-t_1)x'\}$. The E.M.F. of this second cell at t_4, however, is also

$$a_1\{1-(t_4-t_2)x\};$$

so that

$$a_1\{1-(t_4-t_1)x\}=a_2\{1-(t_3-t_1)x'\}.$$

Now, from Clark's experiments and certain preliminary observations made by ourselves, it results that x is approximately equal to x'; whilst if the temperatures are suitably chosen so that the mean of t_1 and t_3 is sensibly the same as the mean of t_2 and t_4, it must result that the difference between x and x' is very small; and, finally, if t_1 and t_3 differ but little in comparison with the difference between t_2 and t_4, any errors in the valuation of x' will be but small relatively. Hence, taking $x=x'$, it results that

$$x=\frac{a_1-a_2}{a_1(t_4-t_2)-a_2(t_3-t_1)}.$$

In order, then, to determine x, it is only necessary to determine the relative readings of two cells, first when one is at t_1 and the other at t_2 (say at 15° and 0° respectively), and secondly when the first is at t_3 and the second at t_4 (say at

14° and 30° respectively), the temperatures being such that t_1+t_3 approximately equals t_2+t_4 (as in the case of the supposed numbers).

To carry out this principle two water-jacketed metal chambers were constructed, furnished with delicate thermometers reading to 0°·01 C., and containing respectively the sets of cells to be compared, the poles of the cells being connected with the mercury-cup arrangement applied to the electrometer by means of covered wires passing through narrow glass tubes fixed in the double lids of the chambers, so that no conducting contact between the wires themselves or between the wires and lids &c. was possible. One of the water-jackets was filled with water at near 15°, the other with water either at or near 0° or at or near 30°, as the case might be; the masses of fluid (agitated from time to time with a peculiar stirrer) were so large that the temperature of the chamber-spaces varied but little during the progress of the series of readings ultimately made. The mean temperatures indicated by the thermometers during the series were taken as the mean temperatures of the cells (placed in the chambers some time before the readings were commenced, so as to attain sensibly the temperatures of the chamber-spaces). The readings were carried out in systematic order; so that the average reading for each cell should be exactly comparable with that of any other, notwithstanding any possible running-down of the electrometer-scale during the progress of the readings. For instance, if in the first chamber two cells (A and B) were placed, and in the second two others (C and D), the readings were alternately taken in the orders A, B, C, D and D, C, B, A, or C, D, A, B and B, A, D, C; so that the average reading for each cell was identical with that which would have been observed had the electrometer-scale value been absolutely constant throughout at its mean value (the actual variation of the electrometer-scale during any set of readings was considerably under 1 per cent.).

Thus, for instance, the following numbers were obtained in two experiments, in each of which the same two cells A and B were placed in the first chamber, and the same two (C and D) in the second:—

	1st experiment.	2nd experiment
t_1	16°·84	17°·04
t_2	1°·08	3°·30
t_3	9°·72	10°·98
t_4	26°·02	25°·12
Average scale-reading for A and B taken together at t_1	159·94	159·62
Average reading for C at t_2	161·00	160·75
Average reading for A and B taken together at t_3 .	153·00	152·56
Average reading of C at t_4	152·25	152·00
a_1 . . .	$\frac{161}{159{\cdot}94}=1{\cdot}0066$	$\frac{160{\cdot}75}{159{\cdot}62}=1{\cdot}0071$
a_2 . . .	$\frac{152{\cdot}25}{153}= {\cdot}9951$	$\frac{152}{152{\cdot}56}= {\cdot}9963$
$x=\frac{a_1-a_2}{a_1(t_4-t_2)-a_2(t_3-t_1)}$ . . .	·000358	·000386

Mean value of x = ·000372.

Similarly the values ·000439 and ·000428 (mean =·000434) were obtained for x in the case of cell D simultaneously examined. The following Table exhibits in brief these figures and those obtained in six other experiments with three other different cells:—

	1st experiment.	2nd experiment.	Mean.
1st cell	·000358	·000386	·000372
2nd „	·000439	·000428	·000434
3rd „	·000480	·000481	·000481
4th „	·000436	·000397	·000417
5th „	·000364	·000336	·000350
		General average . . .	·000411

Hence, finally, it results that the E.M.F. of a Clark's cell set up with saturated zinc-sulphate solution is, at a temperature t not more than 10° or 12° above or below 15°·5 C.,

$$1{\cdot}457\{1-(t-15^\circ{\cdot}5)\times 0{\cdot}00041\} \text{ volt;}$$

it being admitted that Clark's valuation is exact, viz. 1·457 volt at 15°·5.

On the Work done during Electrolysis.

140. The experiments described in the previous portions of these researches have shown that, when a current is passed through an electrolytic cell, the amount of energy expended (positively or negatively) during the passage in performing a given amount of chemical work (apart from that transformed into heat in consequence of the resistance proper of the cell in accordance with Joule's law) is not constant, but *increases algebraically with the current-density*, in such wise that when the cell is an ordinary decomposing cell (*e. g.* a voltameter) the "counter electromotive force" of the cell increases in arithmetical value with the current-density, whilst when the cells is an electromotor (*i. e.* such a cell as to yield a *negative* counter E.M.F.), the arithmetical value of the negative counter E.M.F. (*i. e.* the direct E.M.F. of the cell) decreases with the current-density. The extra work done by a stronger current as compared with a weaker one in the former case, and the deficiency of work corresponding with the fall in direct E.M.F. in the latter case, make their appearance in the form of sensible heat in the cell.

Experiments have been published by Favre (*vide* Part I. §§ 14 and 15) which appear to show that certain forms of electromotor-cells can generate currents capable of doing more work externally to the cell than corresponds with the net chemical action taking place, this extra work being gained at the expense of the sensible heat of the cell, which becomes *cooled* by the passage of a current of too small magnitude to generate, in accordance with Joule's law, sufficient heat in the cell to overpower this cooling action. Inasmuch, however, as the mercury-calorimeter was employed in these experiments of Favre, whilst, from the nature of the case, but feeble currents passed, so that the total amount of chemical action in a given time could be but small, it seems not unlikely that an excessively large probable error attends the numerical values obtained. In point of fact, one of the cells found by Favre to behave in this way was Grove's cell; and his results in this repect are totally at variance with all other experiments on the subject (compare H. F. Weber, Phil. Mag. 1878, v. p. 195), leading to the conclusion that the supposed cooling action was

not a real effect, but simply the result of the accumulation of experimental errors. In order to see whether this was also the case with the other cells examined by Favre, the following experiments were made.

These other cells were simple voltaic couples of zinc and platinum or cadmium and platinum immersed in dilute hydrochloric acid; the numbers obtained by Favre as the cooling effects per gramme equivalent of metal dissolved were respectively 1051 and 1288 gramme-degrees, corresponding with ·046 and ·057 volt*. On the other hand, with dilute sulphuric acid in lieu of hydrochloric, Favre found that no cooling action was traceable, but that the cells were always warmed by the passage of a current. Now these results, if correct, must imply that the E.M.F. of a zinc-platinum or a cadmium-platinum cell, when generating only a minute current, is above the value corresponding with the heat-development due to the net chemical action taking place when hydrochloric-acid solution is the exciting fluid, and below that value when dilute sulphuric acid is used instead; *i. e.* the electromotive forces of cells containing dilute hydrochloric acid must be above ·754 and ·388 volt respectively with zinc-platinum and cadmium-platinum couples, and the electromotive forces of cells containing dilute sulphuric acid must be below ·835 and ·470 volt respectively with these same couples, these being the values in E.M.F. corresponding respectively with the heat-developments per gramme-equivalent in the reactions

$$Zn + 2HCl = ZnCl_2 + H_2,$$
$$Cd + 2HCl = CdCl_2 + H_2,$$
$$Zn + H_2SO_4 = ZnSO_4 + H_2,$$
$$Cd + H_2SO_4 = CdSO_4 + H_2,$$

these heat-developments being, per gramme-molecule, as follows†:—

* For the sake of comparison with the experiments described in the previous portions of these researches, the factor 4410 for converting gramme-degrees into volts is adhered to, notwithstanding that the balance of evidence now seems to indicate that the value of J hitherto assumed (42 megalergs) is somewhat too high, and that the B.A. unit of resistance is upwards of 1 per cent. below its intended value, instead of being exact as hitherto assumed.

† These figures are deduced from Julius Thomsen's thermochemical

$Zn, Cl_2, aq.$	$=112840$
$H_2, Cl_2, aq.$	$=\ \ 78640$
Difference . .	34200
Diff. per gramme-equivalent	17100
Corresponding with volt	·754

$Zn, O, SO_3, aq.$ . . .	$=106090$
H_2, O	$=\ \ 68200$
Difference . .	37890
Diff. per gramme-equivalent	18945
Corresponding with volt	·835

$Cd, Cl_2, aq.$	$=96250$
$H_2, Cl_2, aq.$	$=78640$
Difference . .	17610
Diff. per gramme-equivalent	8805
Corresponding with volt	·388

$Cd, O, SO_3, aq.$.	$=89500$
H_2, O	$=68200$
Difference . .	21300
Diff. per gramme-equivalent	10650
Corresponding with volt	·470

141. In order to see whether the electromotive forces actually developed by these four voltaic combinations are really above the calculated values in the first two cases and below in the second two instances, when the disturbing effects of dissolved air are eliminated, cells were set up like those described in § 85, and caused to generate feeble currents by employing large external resistances. In all cases it was found that when the errors due to dissolved air were eliminated and the readings became constant, the E.M.F. actually developed *invariably fell short of the value corresponding with the net chemical action* by an amount which increased with the current-density until the reduction became a large fraction of the E.M.F. observed with the smallest possible densities. With hydrochloric-acid cells the deficiency was not so great in the first instance, and the rate of increase in deficiency was not so rapid, as with sulphuric-acid cells. Thus the following four experiments may be cited as illustrations of the results obtained in numerous cases:—

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data and the mean value for the heat of formation of water arrived at in § 31. Thomsen's values relate to the degree of dilution $MCl_2, 400\,H_2O$, and $MSO_4, 400\,H_2O$. Some experiments made by us on the amounts of heat evolved on diluting stronger solutions of zinc and cadmium chlorides and sulphates indicate that these values require slight corrections for stronger solutions than those used by Thomsen; but the alterations thus produced in the net heat-development and in the E.M.F. corresponding thereto is but small.

Current, in microamperes, =C.	Current-density, in microampères, per square centimetre.	Observed differences of potential between plates =E.	Value of CR.	E.M.F. of cell, c=E+CR.
Hydrochloric Acid : Zinc and Platinum.				
12·6	1·6	·633	......	·633
23·4	2·9	·628	·001	·629
55·1	6·9	·609	·002	·611
102·4	12·8	·585	·003	·588
224·5	28·1	·545	·007	·552
Calculated E.M.F. .... = ·754				
Hydrochloric Acid : Cadmium and Platinum.				
6·5	0·8	·347	......	·347
11·0	1·4	·291	·001	·292
14·8	1·85	·249	·001	·250
33·7	4·2	·161	·003	·164
54·3	6·8	·130	·005	·135
97·8	12·2	·103	·010	·113
Calculated E.M.F. .... = ·388				
Sulphuric Acid : Zinc and Platinum.				
12·6	1·6	·626	......	·626
23·4	2·9	·540	·001	·541
55·1	6·9	·492	·002	·494
102·4	12·8	·439	·003	·442
224·5	28·1	·353	·007	·360
Calculated E.M.F. .... = ·835				
Sulphuric Acid : Cadmium and Platinum.				
6·5	0·8	·301	......	·301
11·0	1·4	·259	·001	·260
14·8	1·85	·211	·001	·212
33·7	4·2	·080	·003	·083
54·3	6·8	·033	·005	·038
97·8	12·2	·019	·010	·029
Calculated E.M.F. = ... ·470				

In each of these experiments the plate-surface was constantly 8 square centimetres; the hydrochloric-acid solution was close to $2\,HCl, 50\,H_2O$ and $2\,HCl, 100\,H_2O$ in the first and second experiments respectively, and the sulphuric-acid $H_2SO_4, 50\,H_2O$ and $H_2SO_4, 100\,H_2O$ in the third and fourth

experiments respectively. The zinc plates were amalgamated, the cadmium ones not.

142. The experiments described in Parts IV., V., and VI. indicate that the amount of diminution brought about in the E.M.F. of an electromotor (either a simple cell, or one after Daniell's construction) by an increase in the current-density may readily greatly exceed any possible effect due to the accumulation round the two plates of fluids of widely different molecular strength, and, further, that, as a general rule, the effect of diminishing the area of the plate on which the metal is deposited is considerably greater than that of a similar diminution in the area of the other plate, although this is not invariably the case. It is hence evident that the chief source of nonadjuvancy especially lies in the incomplete manifestation as electricity of the energy due, after the elimination by the action of the current of the deposited metal (or body equivalent thereto) in the nascent form, to the subsequent transformation thereof into the permanent form. Clearly the same kind of thing must be equally true for the other products of electrolysis evolved at the other electrode. Hence the reason why a less amount of non-adjuvancy is brought about at this side is presumably the greater amount of attraction exercised by the material of the electrode for the nascent product ("sulphion" of Daniell in the case of cells containing sulphates) here evolved, owing to their opposite chemical characters, than is observable at the other electrode. Admitting this to be so, it should result that the more oxidizable the metal dissolved (*i. e.* the greater the heat of formation of the compound produced by its solution), the less will be the amount of nonadjuvancy due to the incomplete conversion into electricity at this plate of the energy due to transformation of nascent into final products. The results of the experiments hitherto described, however, being complicated by the formation of solutions of different strengths around the two plates, are not sufficiently precise to show that, under given conditions, a zinc plate, for example, causes less nonadjuvancy than a cadmium one, and so on. Accordingly the following experiments on the point were made, the result of which is to show indisputably that the more oxidizable the metal the less the nonadjuvancy.

An electrolytic cell was constructed, consisting of a wide glass tube closed by india-rubber bungs through which passed wires terminating interiorly in the plates to be experimented with, the opposed plate-surfaces being perpendicular to the axis of the tube and therefore parallel to one another, and the anterior portions of the plates and the wires being thickly coated with gutta-percha. The tube was then filled, for instance, with concentrated zinc-sulphate solution, with plates of zinc at an accurately known distance apart, and was kept at a temperature sensibly uniform. A series of currents of various strengths was then passed through the cell, and the difference of potential subsisting between the plates determined in each case. These values represented the numerical values of $e_1 + CR$, where e_1 is the counter E.M.F. set up during the electrolysis, C the current, and R the resistance of the cell; and from them the values of this expression for definite values of C (50, 100, 200 microampères, &c.) were readily calculated by interpolation. The + zinc electrode was then removed, and a copper plate exposing exactly the same area placed in precisely the same position. The observations were then repeated, the temperature being the same as before, and a new series of values, $e_2 + CR$, calculated, e_2 being the counter E.M.F. now set up for a given value of C. Since R is constant throughout, it is evident that the difference between the two values for a given current obtained, first with a zinc, and secondly with a copper + electrode, represents $e_2 - e_1$. Now necessarily both e_1 and e_2 increase with the value of C in accordance with the general law to that effect deduced from all the previous observations (§ 133); but if it be true that a less production of heat instead of electricity is brought about when nascent sulphion is liberated in contact with zinc than when in contact with copper, e_1 must increase less rapidly with the current than e_2, and hence the value of $e_2 - e_1$ *must rise with the current-strength.* Precisely this result was observed in every case: for example, the following numbers were obtained in a pair of sets of observations carried out as described, the area of the plates being 0·50 square centim. throughout.

+ Zinc electrode.		+ Copper electrode.	
C.G.S. current.	Observed potential-difference.	C.G.S. current.	Observed potential-difference.
·00000436	·018	00000466	1·073
·00000866	·039	00000883	1·089
·00001432	·062	00001460	1·127
·00002130	·091	00002275	1·159
·00004160	·174	00004450	1·251

From these figures the following are obtained by interpolation:—

Current.	Potential-difference.		
	+ Zinc.	+ Copper.	e_2-e_1.
·000005	·021	1·075	1·054
·00001	·045	1·101	1·056
·00002	·085	1·147	1·062
·00004	·168	1·232	1·064

Precisely similar results were obtained in numerous other analogous experiments. Thus the following Table illustrates some of the figures obtained, the + zinc plate originally employed being replaced by a plate of the same size, I. of bright copper, II. of electro-copper, III. of amalgamated copper, IV. of bright cadmium, V. of bright silver.

C.G.S. current.	Values of e_2-e_1 obtained				
	I.	II.	III.	IV.	V.
·000005	1·063	1·054	1·065	......	1·486
·00001	1·067	1·057	1·076	......	1·498
·00002	1·073	1·061	1·084	·315	1·503
·00004	1·075	1·068	1·099	·324	1·512

143. A still better illustration of the regular rise in value of e_2-e_1 with the current is afforded by the following series of numbers obtained as the average results of several sets of observations very carefully made—A with a bright zinc +

electrode, B with one of bright cadmium, C with one of bright copper, and D with one of bright silver. In every case the mean temperature was the same within two or three tenths of a degree (varying from 17°·55 to 17°·9 throughout). In the last case it was found that, whilst perfectly steady readings could be obtained with current-strengths up to something like ·0007, with higher strengths this was no longer the case, silver peroxide being apparently formed instead of silver sulphate. In these experiments all the plates exposed an area of 1·5 square centim., the solution electrolyzed being a nearly saturated one of pure zinc sulphate, renewed for each series; the plates were about 5 centim. apart, the tube holding them being 3 centim. in internal diameter.

C.G.S. current.	Difference of potential set up.			
	A.	B	C.	D.
·00002	·029	·317	1·069	1·490
·00005	·044	·334	1·086	1·509
·0001	·063	·354	1·107	1·530
·0002	·084	·381	1·139	1·562
·0005	·146	·451	1·210	1·636
·001	·230	·547	1·310	
·0015	·311	·636	1·403	
·002	·389	·730	1·498	
·0025	·476	·830	1·598	

These figures yield the following six sets of values of e_2-e_1 for the corresponding pairs of + electrodes compared.

Current.	Zinc-cadmium.	Zinc-copper.	Zinc-silver.	Cadmium-copper.	Cadmium-silver.	Copper-silver.
·00002	·288	1·040	1·461	·752	1·173	·421
·00005	·290	1·042	1·465	·752	1·175	·423
·0001	·291	1·044	1·467	·753	1·176	·423
·0002	·297	1·055	1·478	·758	1·181	·423
·0005	·305	1·064	1·490	·759	1·185	·426
·001	·317	1·080	......	·763		
·0015	·325	1·092	......	·767		
·002	·341	1·109	......	·768		
·0025	·354	1·122	......	·768		

Not only does the value of e_2-e_1 increase with the current-density in every case, but, further, the rate of increase is greater when zinc is compared with silver than with copper, and greater then than when compared with cadmium; similarly the rate of increase with cadmium and silver is greater

than with copper and silver, and so on. It is further noticeable that in each case a value of e_2-e_1 with some particular current-strength is deducible which is sensibly the same as the E.M.F. of a cell after Daniell's construction containing the same metals and sulphate solutions of equal molecular strength; so that in general it may be said that, for a current-density below a particular limit, the value of e_2-e_1 is less than that of the corresponding Daniell form of cell, whilst for a current-density above this limit it is greater.

144. The following experiment seems to show that the substitution of dilute sulphuric acid for zinc-sulphate solution as the electrolyte makes no material difference in the end result, the $-$ electrode being made of platinum, and the disturbing influence of dissolved air being eliminated. Two precisely similar U-tube cells (§ 85) were filled with recently boiled dilute sulphuric acid (11·5 grammes $H_2\,SO_4$ per 100 cubic centim.), and fitted with uniformly sized plates (8 square centim. total surface in each case) at an equal distance asunder, so that the resistance of the cell should be sensibly the same in each case. In the first cell the plates were of zinc (amalgamated) and platinum, and in the second of copper and platinum respectively; the two were arranged in series with a couple of Leclanché cells, so that the platinum plates were necessarily the $-$ electrodes; a large variable resistance being included in the circuit, the current could be regulated at pleasure. A current of some fifty microampères being sent through for three days, the readings became steady when all the dissolved air around the platinum plates was eliminated; the current was then varied from time to time, and a series of readings of the potential-difference between each pair of plates taken. By interpolation as before, the following figures were then deduced from the average values.

Current in micro-ampères.	Micro-ampères per square centim.	Difference of potential.		e_2-e_1.
		+ zinc.	+ copper.	
20	2·5	$-$·552	+·449	+1·001
40	5·0	$-$·558	+·448	+1·006
80	10·0	$-$·498	+·521	+1·019

The value of e_2-e_1 consequently increases with the current-density as before. The numerical values observed in this experiment are somewhat lower than those found in the experiments above described, as might be expected, since the largest current-density employed in this case, being only 10 microamperes per square centim., is considerably below the smallest cited in the previous observations, in the last of which a minimum current of ·00002 C.G.S. units (or 200 microampères) was employed with plate-surfaces of 1·5 square centim., giving a density of 133·3 microampères per square centim., in which case the value of e_2-e_1 was 1·040; whilst in the former experiments a minimum current of ·000005 C.G.S. unit (50 microampères) was employed with a plate-surface of ·50 square centim., giving a density of 100 microampères per square centim., when values of from 1·054 to 1·065 were observed.

145. Some experiments were also made with analogous pairs of cells in which the + electrodes were made of metals not attacked by the nascent products arising from the electrolysis of sulphates, *e. g.* gold and platinum. In these instances it was found that platinum behaved in reference to gold just as a more readily to a less readily oxidizable metal, this result being evidently brought about by the superior surface condensing-power possessed by platinum, in virtue of which a greater proportion of the energy due to the transformation of the nascent into the final products of electrolysis evolved at the + electrode becomes adjuvant. For instance, the following numbers were obtained with a pair of precisely similar cells containing the same copper-sulphate solution and copper − electrodes.

Current-density, microampères per square centimetre.	Difference of potential between plates.		Difference.
	+Platinum.	+Gold.	
3·0	1·500	1·555	·055
7·0	1·534	1·591	·057
11·0	1·570	1·630	·060

Even with the lowest current-density and with platinum as

+ electrode the total amount of nonadjuvancy was here considerable; for the E.M.F. corresponding with the net chemical action is only 1·234 volt ($\frac{1}{2}$[Cu, O, SO_3 aq] = 27,980 gramme-degrees = 1·234 volt); and the minimum difference of potential set up, after correction for the resistance of the cell (*i. e.* the counter E.M.F. set up, or the value of the term e in the expression $E = e + CR$), exceeds 1·490, since the term CR in this case was much less than ·010 volt.

In just the same kind of way, when platinum and gold were respectively made the − electrodes in similar pairs of cells containing dilute sulphuric acid and a constant oxidizable + electrode, the superior surface condensing-power possessed by platinum caused a less degree of nonadjuvancy during the transformation of nascent into free hydrogen. Thus, for example, the following numbers were obtained with a copper + electrode and acid containing 10 per cent. of H_2 SO_4.

Current-density, micro-ampères per square centimetre.	Difference of potential between plates.		Difference.
	−Platinum.	−Gold.	
2·5	·449	·575	·126
5·0	·488	·619	·131
10·0	·521	·661	·140

Here again, even in the most favourable instance, with the smallest current-density and platinum as − electrode, a conderable amount of nonadjuvancy subsisted; for the value of CR in this case was not greater than ·001; so that the minimum counter E.M.F. set up was at least ·448 volt, whilst the E.M.F. corresponding to the net chemical action is only ·270 volt, the heat-development being per gramme equivalent

$$\begin{array}{lrll} \frac{1}{2}(H_2, O) \quad . \quad . & = & 34100 & \text{gramme-degrees.} \\ \frac{1}{2}(Cu, O, SO_3\,aq) & = & 27980 & \text{,,} \quad \text{,,} \\ \hline & & 6120 & \text{,,} \quad \text{,,} \quad = \cdot 270 \text{ volt.} \end{array}$$

It is hence evident, *à fortiori*, that when acidulated water is decomposed with two gold electrodes, the counter E.M.F. set up must be much greater for a given current-density than

when two platinum electrodes are used, the deficiency in condensing-power being then manifest at both electrodes simultaneously. The experiments described in Part IV. § 90 have shown that this is the case.

146. In addition to the experiments above described as examples, a large number of analogous observations have been made with varying kinds of electrolytic solutions and electrodes, and with varying strengths of solutions. The general results of these experiments, so far as at present completed, may be thus summarized.

(1) When an electrolytic cell is of such a nature that the counter E.M.F. set up is negative (*i. e.* when the cell is an electromotor), it is always found that *the E.M.F. developed is less the greater the density of the current generated.* With very small current-densities the E.M.F. has a maximum value which in certain cases (*e. g.* Daniell's cell and the analogous zinc-cadmium and cadmium-copper cells described in Part VI.) is substantially identical with the E.M.F. corresponding with the heat-development due to the net chemical action taking place in the cell, *i. e.* with the E.M.F. representing the algebraic sum of the chemical affinities involved. In certain other cases (*e. g.* the zinc-silver, cadmium-silver, and copper-silver cells described in Part VI.) the maximum E.M.F. developed is sensibly *below* that due to the net chemical action.

(2) Some kinds of combinations have been found to be capable of existing which can develop a *greater* E.M.F. than that due to the net chemical action (although the particular cells described by Favre as possessing this property are not really cases in point, Favre's results being due to experimental errors); amongst such combinations may be mentioned several where *lead* is the metal dissolved, *i. e.* lead-copper cells charged with solutions of acetates. It is noticeable that in such cases Volta's law of summation holds, the sum of the electromotive forces of two cells, one containing zinc and lead and the other lead and copper, being equal to the E.M.F. of a zinc-copper cell, the E.M.F. of the first cell being just as much below the amount calculated from the heat-development as that of the second is above the amount similarly calculated. This class of cells is now undergoing careful examination, and will be dealt with in a subsequent paper. Unfortunately,

progress in this direction during the last fifteen months has been greatly retarded by the refusal of the Administrators of the Government Fund of £4000 to continue the grants by the aid of which the previous portions of these researches have mainly been made, on account of which circumstance numerous other points of interest that have cropped up have necessarily remained uninvestigated *.

(3) When the electrolytic cell is not an electromotor, the counter E.M.F. set up (positive) always *increases in amount with the current-density*. When the + electrode is of such a nature as to combine with the products of electrolysis evolved thereat, other things being the same, *the rate of increase is slower the greater the chemical affinity* between the nascent products of electrolysis evolved at the + electrode and the material of which that electrode is composed; *i. e.* the greater the affinity, the less the degree of nonadjuvancy brought about at the + electrode.

(4) Whether the cell be an electromotor or not, there is always (with currents not so small as to be practically infinitesimal) a greater or less degree of nonadjuvancy brought about at the — electrode, owing to the development of heat in lieu of electricity during the transformation of nascent into ultimate permanent products of electrolysis. In many cases this source of nonadjuvancy decidedly predominates over that at the + electrode.

(5) The particular extent to which the nonadjuvancy reaches at either electrode appears to be a complex function not only of the chemical nature of the electrode, the physical conditions of its surface, and the character of the nascent products of electrolysis evolved thereat, but also of the temperature, and the degree of concentration of the solution electrolyzed, and possibly of other conditions besides. Other things being equal, it appears to be a general rule that *the weaker the solution, the greater the degree of nonadjuvancy*. When a gas is

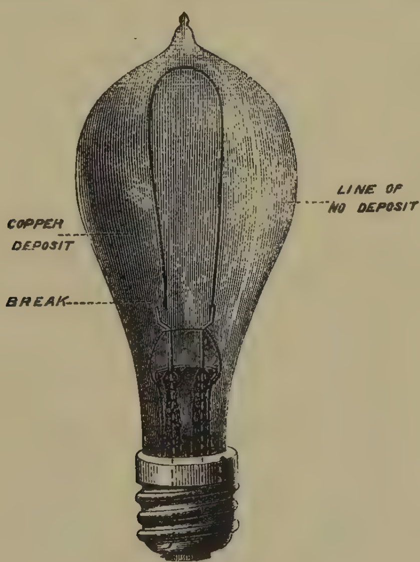
* Since the presentation to the Physical Society of Part VI. of these researches, a paper has appeared by F. Braun (*Annalen der Phys. u. Chem.* xvi. p. 561), in which the author shows that various combinations examined by him give electromotive forces sensibly the same as those calculated from thermochemical data, whilst others fall short of, and some exceed, the calculated values.

one of the permanent products of electrolysis at either electrode, *the greater the surface condensing-power of the material of which the electrode is composed, the less is the degree of non-adjuvancy.*

XXXI. *On a Phenomenon of Molecular Radiation in Incandescence Lamps.* *By* J. A. Fleming, *B.A., D.Sc.**

Not long ago a curious phenomenon came under my notice in connexion with the burning of Edison incandescence lamps, which presents sufficient interest to warrant my drawing the attention of physicists to it.

As is well known, the carbon filament in the Edison lamp is of a horse-shoe form. The two extremities of the loop are



clamped into small copper clamps on the ends of the platinum wires, which are sealed through the glass. The ends of the carbon loop are electroplated over with copper at the place where they are connected to the clamp in order to make a good contact. If this precaution is omitted, a loose contact

* Read May 26, 1883.

may be formed, the result of which will be a generation of heat at that point.

In the ordinary working the life-history of a carbon filament is something as follows:—

At some point or other the filament is probably thinner than at other places. At this place there will be a greater generation of heat and a higher temperature; volatilization of the carbon ensues, and the vapour condenses on the sides of the glass bulb, as far as I have observed, uniformly. If, however, the point of greatest resistance occurs on the copper clamp, then it is found that copper volatilizes and deposits on the inside of the glass.

But what is most curious is, that in this case an examination of the glass envelope shows that there is a narrow line along which no copper has been deposited. This is seen best by holding the lamp up before the light and slowly turning it round. In one particular position, easily found, it is best seen. Now, on examining carefully the position of the line of no deposit as compared with the position of the carbon filament, it will be seen that it lies in the plane of the loop, and on the opposite side to that nearest to which the break of the loop has occurred. It is in fact *a shadow of the loop*.

The conclusion which must be arrived at, then, is that the copper molecules are shot off in straight lines; otherwise it is impossible that there should be this line of no deposit.

The most noticeable thing is, that it occurs only when the deposition of copper takes place; I have never noticed it in an ordinary carbon deposit.

Hence there must be some essential difference between the vaporization of the carbon and that of the copper. The carbon deposit resembles more the condensation of a vapour and is uniformly distributed; but the copper deposit exhibits the character of a molecular radiation or shower taking place from a certain point.

The whole phenomenon calls at once to mind the beautiful researches of Mr. Crookes with vacuum-tubes. Here, however, we are dealing not with an induction-coil discharge, but with a comparatively low potential.

I have never failed to see the effect in any lamp which has had a deposition of copper on its interior.

It is interesting to note how nearly the colour of transparent copper resembles that of transparent gold. The similarity of the surface-colour of pure unoxidized copper and of gold is accompanied by a near resemblance in colour of the two metals in thin films.

XXXII. *An Illustration of the Crossing of Rays.* *By* Walter Baily*.

[Plate XII.]

When rays of light are passing through a point, the resultant motion of the æther is in general far too complicated to be conceived; but if the light is homogeneous, it can readily be shown that the motion at each point is simply harmonic motion in an ellipse; so that in that case the complication consists only of the change in this ellipse in passing from one point to another. Hence a model might be constructed to represent the crossing of homogeneous rays by placing a number of ellipses to represent the motion at a number of separate points, through which the light might be supposed to be passing. If we further simplify the case by considering only rays parallel to one plane, and suppose them to be plane-polarized so that the vibrations are parallel to the same plane, the whole motion will be parallel to that plane, and might be represented by means of diagrams.

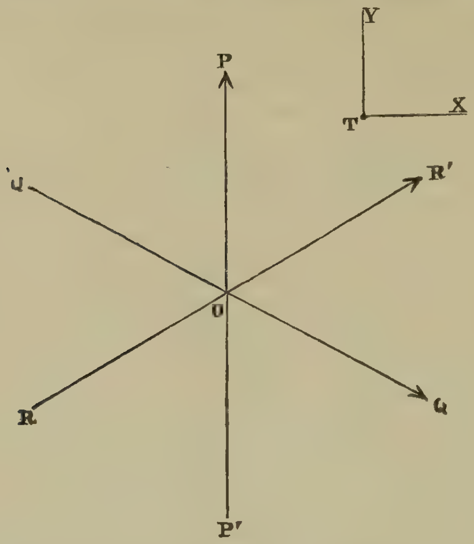
The case worked out in this paper is that of three rays of equal intensity parallel to one plane, plane-polarized so that the vibrations are parallel to that plane, and meeting one another at equal angles.

Take any point O, and let P′ O P, Q′ O Q, R′ O R be the rays through O. Take any other point T in the same plane; draw T X, T Y perpendicular and parallel respectively to P′ O P. Let p, q, r be the distances from O of the feet of the perpendiculars drawn from T on P′ O P, Q′ O Q, R′ O R respectively; these distances being considered positive if drawn towards P, Q, R, and negative if drawn towards P′, Q′, R′. Then it may be shown that

$$p+q+r=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

* Read May 26, 1883.

The position of T may be defined by any two of these quantities. The equations $p=$ const., $q=$ const., $r=$ const., are equa-



tions to straight lines perpendicular to P′ O P, Q′ O Q, R′ O R respectively; and the equations $q-r=$ const, $r-p=$ const., $p-q=$ const. are equations to lines parallel to P′ O P, Q′ O Q, R′ O R respectively. When the constant is zero, the lines pass through O.

If we take any point in Q′Q and move perpendicularly to Q′ Q from this point, we can, without altering the phase of the vibration of the ray Q, reach a point at which the phase of the vibration of the ray R is the same. If we now move from this latter point in a direction parallel to P′ P, we shall keep the phases of Q, R equal to one another, and we can reach a point at which the phase of the ray P is equal to either of them. Take this point as the origin, and let the phases be zero at the initial time. Then at a time t the displacements due to the three rays at the point T will be $\sin 2\pi(t-p)$, $\sin 2\pi(t-q)$, $\sin 2\pi(t-r)$, the wave-length being taken as the unit of length, and the period as the unit of time.

Let x be the amount of displacement along TX and y that along TY, at the time t. Then

$$x = \sin\frac{\pi}{2}\sin 2\pi(t-p) + \sin\left(\frac{\pi}{2}+\frac{2\pi}{3}\right)\sin 2\pi(t-q)$$
$$+ \sin\left(\frac{\pi}{2}-\frac{2\pi}{3}\right)\sin 2\pi(t-r),$$
$$y = \cos\frac{\pi}{2}\sin 2\pi(t-p) + \cos\left(\frac{\pi}{2}+\frac{2\pi}{3}\right)\sin 2\pi(t-q)$$
$$+ \cos\left(\frac{\pi}{2}-\frac{2\pi}{3}\right)\sin 2\pi(t-r).$$

By means of (1) these equations may be written

$$x = \sin 2\pi(t-p) - \cos\pi(q-r)\sin 2\pi\left(t+\frac{p}{2}\right), \quad . \quad (2)$$
$$y = \sqrt{3}\sin\pi(q-r)\cos 2\pi\left(t+\frac{p}{2}\right). \quad . \quad . \quad . \quad . \quad . \quad (3)$$

In general the calculation of the phase and the ellipse would be laborious; but it may be readily effected along lines parallel to P′OP, Q′OQ, R′OR at distances $\frac{1}{\sqrt{3}}$ from one another as follows:—We have as equation to such lines parallel to P′OP, $q-r=n$, where n is an integer. Hence

$$y=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$
$$x = \sin 2\pi(t-p) - \sin 2\pi\left(t+\frac{p}{2}\right)\cos n\pi.$$

If n is even,

$$x = -\sqrt{2-2\cos 3\pi p}\,.\cos 2\pi\left(t-\frac{p}{4}\right). \quad . \quad . \quad (5)$$

If n is odd,

$$x = \sqrt{2+2\cos 3\pi p}\,.\sin 2\pi\left(t-\frac{p}{4}\right). \quad . \quad . \quad . \quad (6)$$

Equation (4) shows that along these lines the vibrations are rectilinear, and perpendicular to direction of the ray.

Putting $p=\frac{m}{3}$, m being an integer, we see from (5) and (6) that there are points of no motion when m and n are both even or both odd. These conditions will be satisfied if p, q, and r are multiples of $\frac{1}{3}$. In order to satisfy (1), one of the quantities must be an even multiple, and the other two must be both even or both odd.

We may obtain similar equations in relation to Q′OQ and R′OR; and the points of no motion will be the same as those already obtained. If we draw the three sets of lines above

considered, we shall form a series of triangles whose sides are parallel to the rays, each side being equal to $\frac{2}{3}$. These triangles will have the properties, that their angles will be nodes, and that the vibrations along their sides will be perpendicular to the sides, the displacement being given by equations (5) and (6) and the corresponding equations for the rays Q and R. The form of these triangles under displacement, when $t=0$, is shown in Pl. XII. fig. 1.

The motion may be also readily obtained along lines perpendicular to the direction of the rays, at distances $\frac{1}{3}$ from each other, one of each set passing through the origin. p must be a multiple of $\frac{1}{3}$; and there are six different forms of equations (2) and (3) for six consecutive values of p, which are given in the following Table (n being an integer):—

p.	x.	y.
$2n+1$	$(1+A)\sin 2\pi t$	$-B\cos 2\pi t$
$2n+\frac{2}{3}$	$(1-A)\sin 2\pi(t+\frac{1}{3})$	$+B\cos 2\pi(t+\frac{1}{3})$
$2n+\frac{1}{3}$	$(1+A)\sin 2\pi(t-\frac{1}{3})$	$-B\cos 2\pi(t-\frac{1}{3})$
$2n$	$(1-A)\sin 2\pi t$	$+B\cos 2\pi t$
$2n-\frac{1}{3}$	$(1+A)\sin 2\pi(t+\frac{1}{3})$	$-B\cos 2\pi(t+\frac{1}{3})$
$2n-\frac{2}{3}$	$(1-A)\sin 2\pi(t-\frac{1}{3})$	$+B\cos 2\pi(t-\frac{1}{3})$

where $$A=\cos\pi(q-r),\quad B=\sqrt{3}\sin\pi(q-r).$$

These lines intersect the triangles (fig. 1) at their angles, and also at the bisection of their sides. At these points the motion has been already determined. The motion is circular if p is an even multiple of $\frac{1}{3}$, at the points for which $1-A=\pm B$ —that is, where $q-r=2m\pm\frac{2}{3}$ (m being an integer); and if p is an odd multiple of $\frac{1}{3}$, at the points for which $1+A=\pm B$ —that is, where $q-r=2m\pm\frac{1}{3}$.

These conditions are satisfied at the middle points of the triangles. In fig. 2 are shown the nodes and the circular points, the arrows indicating the phase when $t=0$. It will be noticed that at adjacent circular points the motion is in opposite directions.

It would be possible to construct a piece of apparatus to exhibit the motion approximately. A piece of elastic membrane, sufficiently stretched in all directions, should be fastened at a set of points corresponding to the points of rest, and the middle points of the triangles should then be displaced according to the phase (see fig. 2), and carried round their original positions in circles of equal size and period, the adjacent motions being in opposite directions—an arrangement which might easily be effected by a series of cogged wheels. We should then have a number of points fixed, and the correct motion given at other points where the motion is greatest. The motion of the rest of the membrane except near the edges would then be approximately correct.

In fig. 3 is given an enlarged view of one of the triangles, showing some of the points where the motion is elliptic, and the displacement of the lines through the nodes parallel and perpendicular to the rays.

XXXIII. *Improved Construction of the Movable-coil Galvanometer for determining Current-strength and Electromotive Force in Absolute Measure. By* Dr. EUGEN OBACH*.

SOME years ago I showed that the tangent-galvanometer of ordinary dimensions may be employed as a measuring instrument for very strong currents if the ring is made movable around its horizontal diameter †, a principle already adopted before that time by Prof. Trowbridge, of Harvard College, Massachusetts ‡; a little later I described a galvanometer based upon that principle, and constructed by Messrs. Siemens Brothers §.

I now propose to give a brief account of several alterations which have since been introduced by that firm, and which I venture to think render the instrument more sensitive and more convenient for use, besides creating for it a wider field.

* Read June 9, 1883

† 'Nature,' xviii. p. 707 (1878); *Repertor. für Exper. Physik.* xiv. p. 507 (1878).

‡ Amer. Journ. of Arts and Science, vol. ii. (August 1871).

§ *Zeitschrift für angewandte Electricitätslehre*, i. p. 4 (1879).

As the galvanometer, in the complete form in which I shall presently describe it, is not so much destined to meet the daily want of the practical electrician, but is rather intended for measurements where greater accuracy and trustworthiness than usual is necessary, I thought myself justified in bringing the subject before the Physical Society, particularly as the kindness of Messrs. Siemens Bros. & Co. at the same time enables me to place the instruments before you for inspection.

Ere proceeding further, allow me to say that I shall not on this occasion touch upon the theory of the instrument, which is already given elsewhere, but confine myself wholly to describing the recent improvements in its construction, adding a few series of measurements in order to prove the high degree of accuracy obtainable.

I propose to deal with the different parts of the instrument under separate headings; and will first speak of

THE MAGNETIC NEEDLE AND ITS POINTER.

The older instruments had a flat magnetic needle fixed to a light vertical axle, pivotted at both ends between jewels to prevent any dipping, which the needle would otherwise expe-

Half nat. size.

rience with great inclinations of the ring. This arrangement answered sufficiently well with ordinary care; but still the

delicate pivots were likely to be damaged, thus impairing the sensitiveness of the needle. As now constructed, the dipping of the needle is completely avoided in the manner illustrated by the annexed figure. The needle, *n s*, is fixed to a thin vertical axle, *a b*, near its upper end, the lower end of the axle being provided with a cylindrical brass weight, *w*. This weight offers but little additional momentum to the whole system round the vertical axis, whereas the movement round the horizontal axis is completely prevented. The aluminium pointer, *p q*, is situated in the same plane as the scale; the ends are flattened and provided with a fine slit, which serves as an index for reading the deflections, the bottom of the needle-box being blackened. The reading can thus be taken without parallax, and therefore very accurately. The magnetic needle has a biconical shape, which entirely prevents the shifting of the magnetic axis from its original position, as was sometimes found to be the case with the old broad needles. Adjustments are provided by which the cocoon-fibre, *f*, serving to suspend the needle, can be raised or lowered, as well as accurately centred.

The Damping of the Oscillations.

Numerous experiments were undertaken to ascertain a convenient method for damping the oscillations of the needle, and to arrive, if possible, at a perfectly aperiodical movement. After trying large masses of copper placed in the immediate neighbourhood of the swinging magnet, as well as liquid damping, without decided success, air-damping was resorted to, and finally adopted. It will be remembered that Sir William Thomson used air-damping for the light-mirror of his dead-beat galvanometer, and Prof. Töpler * for other galvanometric apparatus. In our case the air-chamber consists of a shallow cylindrical box, about 8 centim. in diameter, $1\frac{1}{4}$ centim. high, provided with two radial partitions which can be slid in or out; the axle of the needle, passing through the centre of this box, carries a light and closely fitting vane. By sliding the partitions more or less into the box various degrees of damping can be obtained; and if they are right in, the motion is practically dead-beat.

* *Repert. f. exp. Phys.* ix. p. 259 (1873)

The Scales.

Declination-scale.—This scale, engraved on a horizontal ring, was formerly divided into degrees, as usually done; but now one semicircle is provided with divisions corresponding to the natural tangents. The interval between each two divisions must of course vary for different parts of the scale, and is arranged as follows:—

Table I.

Values of tangent.	Interval.
0 to 1	0·01
1 „ 2	0·02
2 „ 3	0·05
3 „ 5	0·10

It will be noticed that the value of the interval only changes at those places where the tangent is equal to a whole figure, thus making a mistake in reading less likely Looking at this scale no gaps are conspicuous, and the divisions are everywhere pretty evenly distributed. Tangent-scales have been employed by Joule, Sir William Thomson, and others; but the one now described seems well to satisfy all the requirements.

Inclination-scale.—This scale, engraved on a vertical quadrant divided into degrees, can accurately be read to one tenth by means of a vernier. The zero division was formerly that to which the index pointed when the ring was horizontal. In this case the tangent of the deflections had to be divided by the sine of the angles. For convenience' sake, the places were specially marked on the scale at which the sines corresponded to whole figures. The new inclination-scale has the zero at the vertical or normal position of the ring; and instead of the sines, the *secants* are specially marked which are represented by whole figures. With these secants the tangents of the deflections must be multiplied; and they can therefore be termed *multiplying powers,* analogous to the multiplying power of shunts. The instruments intended only for the measurement of current-strength have the quadrant bearing the secant scale fixed outside the ring, whilst the others, measuring also electromotive force, have it situated between the needle-box and the ring, where it is better protected from injury.

If the deflections of the needle are read on the tangent-

scale and the positions of the ring on the secant-scale, the aid of trigonometrical tables may be entirely dispensed with, as the product of the two figures represents the quantity to be measured, irrespective of a constant.

The Solid Ring and the Coil.

If the galvanometer has to serve only for the measurement of currents, the gun-metal ring is of a rectangular cross section; but if it is at the same time destined to measure difference of potential, the cross section is V-shaped, the groove being filled with numerous turns of G.S. wire. If the number of convolutions is known, and if a simple relation exists between that number and the resistance of the wire, a great advantage may be derived therefrom. For instance, if there are one thousand convolutions on the coil, offering a resistance of exactly one thousand ohms, the current due to the difference of potential of one volt at the ends of the coil would produce the same deflection of the needle as the current of one ampère flowing through the solid metal ring. That this must be so is evident, if it is remembered that the weak current of one thousandth of an ampère flows round the needle one thousand times, but the stronger current of one ampère only once. The solid ring and the convolutions are thus arranged that their cross sections have a common centre of gravity, thus both acting exactly in the same way upon the magnetic needle. If this simple plan is adopted, the calibration of the galvanometer for difference of potential in volts, which is readily performed with a few cells of known E.M.F., at the same time gives the graduation of the instrument for strength of current in ampères. I have been using various modifications of the Daniell cell with solutions of copper and zinc sulphate of equal specific gravity. At present I am engaged in constructing a standard cell for such purposes, which is always at disposal; and, as far as the preliminary experiments show, the E.M.F. of the new cell will closely approach one volt. Further on I shall communicate some measurements, by which I intend to show how accurately the calibration of the fine-wire coil in volts can serve for that of the solid ring in ampères.

The Constant Shunt and Leading Wires.

With the size of the ring usually employed, viz. 30 centim.

diameter, and our horizontal component of the earth's magnetism, currents of greater strength than about 50 ampères would require the ring to be at the multiplying powers 9 or 10, *i. e.* near the horizontal position. If, as a rule, such currents have to be measured, it is desirable to raise the constant of the galvanometer, say two or threefold, without a proportionate increase of the dimensions. This can be done by the use of a so-called "*constant shunt*," thereby allowing only half or one third of the current to flow round the needle. In our case the shunt is made of exactly the same metal as the solid ring itself; it has no soldering-places, and consists in fact of three or four little bridge pieces left standing instead of cutting the ring quite open where the terminals join. By comparison with an instrument of the same description having an open ring, the shunt can be adjusted to any power desirable.

However, by the introduction of the "constant shunt" the accuracy of the measurements is somewhat impaired. Experiments in which the shunt-pieces were touched with a thin stick of low-melting material during the passage of very strong currents, proved that they did not become hot, on account of their extremely low absolute resistance and their contact with the large metallic mass of the ring conducting away the heat. Variations of temperature, to which both the ring and the shunt are subjected, do not of course in the least disturb the ratio of their resistance, since they both consist of the very same alloy.

As the current only passes round the needle once and, if powerful enough, produces deflections even if the ring is almost horizontal, it is hardly necessary to call attention to the fact that the wires leading the current to the instrument should be so arranged that they cannot act upon the needle; still I have seen instances where this simple and almost self-evident precaution has been strangely neglected. I thought it therefore best to have special leading wires provided which are absolutely inactive upon the needle, and may therefore be named "*adynamic leads*." These leads consist of a number of well-insulated copper wires stranded together in a peculiar manner, and covered with a cotton braiding, similar to the ordinary speaking-tubes. The cable thus formed is quite

flexible, and without the slightest action upon a magnetic needle. I sent a strong current through several turns of such a cable, and held it close to a delicately suspended magnetic needle, but could not detect any effect whatever upon it. One half of the wires is covered with a differently coloured material to the other half; and the wires of each colour are united at both ends of the cable, and there soldered to a stout piece of copper wire. The adynamic cable can be made in any length and for different current-strengths; and as it offers only a small resistance, it can be employed to convey the current to be measured to a locality where the needles are not disturbed by machines or wires.

The Adjusting Parts and the Compensating Magnet.

With regard to the adjustment of the instrument, it suffices to say that, besides the necessary levelling-arrangements, it is provided with clamp-rings for tightly holding the pillar as well as the movable coil without interfering with previous adjustments, and in the same manner as is often done with mathematical apparatus. The final adjustment of the axis of the coil into the meridian is performed by means of a fine screw, which proves very useful for correcting, during a series of measurements, the occasional variations of the zero position.

As long as the galvanometer stands in the same position, the "*constant*," as a rule, changes but little even from day to day. If, however, the instrument is taken from one place to another, great changes in that respect will occur, amounting sometimes to many per cents. These changes do not of course interfere with the accuracy of the measurements, because the "constant" can easily be redetermined with a known E.M.F. at any place, as we have seen; but it would undoubtedly be very convenient to have the galvanometer of *equal sensibility* everywhere. For that purpose an auxiliary magnet is placed east or west from the needle in a plane parallel to the meridian, which can turn round a horizontal axis passing through its neutral point and the centre of the needle, and being at right angles to the diameter on which the coil is turned. This magnet does not affect the zero position, and moreover, if placed exactly vertical with its magnetic axis, it does not alter the original constant, which then only depends upon the

horizontal terrestrial component, more or less modified by the surroundings; but if it is dipped, the horizontal force acting on the needle is either augmented or diminished, according to the direction in which the magnet is turned and to the amount of dip given. It is easily seen that the magnetic influence of the surroundings upon the needle may now greatly vary from one place to another and still be compensated by the magnet, thus keeping the so-called "constant" of the galvanometer actually at a *constant value*, adjusting it, for instance, always so that the unit deflection of 45° with the vertical ring corresponds to a round number of volts and ampères, say five or ten. Under such conditions the deflection-scale could at once give the current or E.M.F. in ampères or volts. A gradual change of magnetism in the compensating magnet does not affect the measurements. I shall not further enter into this subject here, as I intend discussing it more fully on a future occasion.

Methods of Measurement.

For measuring current-strength or electromotive force either of the following four methods can be employed according to circumstances, viz.:—

I. *The General Method.*—Applicable under almost any conditions. The coil is placed in such a position that the deflection attains a proper value. If α is the deflection of the needle, and ϕ that of the coil from the meridian, the formula is

$$x = \tan\alpha \times \sec\phi \times \text{const.};$$

or in case the multiplying powers P on the quadrant are used, the formula becomes

$$x = \tan\alpha \times P \times \text{const.}$$

II. *Method of Equality.*—With this method the coil is each time placed in such a position that the needle is deflected exactly to the *same angle* ψ to which the coil is inclined, giving the formula

$$x = \tan\psi \times \sec\psi \times \text{const.}$$

Having then only to deal with a single angle for a particular measurement, these products of tangents and secants may be

calculated beforehand. For this purpose the natural sines are sufficient, because $\tan \times \sec = \frac{\sin}{\cos^2}$.

The following Table gives, for easy comparison, the values of tangents, secants, and their products at ten and multiples of ten degrees. These products, like the tangents, range from nil to infinity, but increase more rapidly.

TABLE II.

Angle.	0°	10°	20°	30°	40°	50°	60°	70°	80°	90°
tan	0	·176	·364	·577	·839	1·192	1·732	2·747	5·671	∞
sec	1·0	1·015	1·064	1·155	1·305	1·556	2·000	2·924	5·759	∞
tan × sec	0	·1786	·3873	·6664	1·095	1·855	3·464	8·032	32·66	∞

III. *Method of Constant Deflection.*—Here the coil is each time inclined until the needle reaches a certain deflection, say $26\frac{1}{2}°$, $45°$, or $63\frac{1}{2}°$, of which the corresponding tangents are $\frac{1}{2}$, 1, and 2 respectively. This figure then enters the constant, giving the simpler formula

$$x = \sec \phi \times \text{const.},$$

the instrument acting as a *secant-galvanometer*. For a given constant deflection the secant-measurements range between unity and infinity, as the above little table shows. This method has the peculiarity that the needle occupies a fixed position in space during the measurements, which in some instances may be found of advantage.

IV. *Method of Constant Inclination.*—In this case the coil is fixed at a given inclination, and $\sec \phi$ enters the constant; thus the formula is reduced to that of the ordinary tangent-galvanometer,

$$x = \tan \alpha \times \text{const.}$$

As compared with other galvanometers proposed for a similar purpose, the one here described offers the great advantage that the magnetic needle has not to be shifted from one measurement to another, whereby the magnetic field may

sometimes considerably alter; furthermore it does not depend upon the constancy of permanent magnets, which, to say the least, is rather precarious*.

Numerical Results of Measurements.

I shall now communicate some measurements and tests to which the latest forms of instruments have been subjected, in order to illustrate the degree of accuracy obtainable. The first set was undertaken to ascertain the relation actually existing between the solid ring for currents and the coil of wire for E.M.F., which, it will be remembered, was intended to be such that ampères with the solid ring should accurately correspond to volts with the wire coil. The experiment was conducted as follows:—A current, from my constant battery with acid flow†, was sent through the solid ring and a copper voltameter in circuit for a certain time, the deflections to the right and left being observed every five minutes. The mean of these deflections was taken as corresponding to the amount of copper deposited. The copper-sulphate solution and the electrodes consisted of pure materials. The mean of the gain of the kathode and the loss of the anode was taken. The amount corresponding to one ampère per hour is 1·164 gramme of copper or 3·96 grammes of silver, the latter figure being that adopted by Messrs. Siemens and Halske, of Berlin‡. The calibration of the fine-wire coil for volts was performed by means of a number of Daniells, each compared with a Raoult's standard cell filled with pure sulphate solutions and having the E.M.F. 1·115 volt, according to Dr. Alder Wright's experiments §. The figures obtained were as follows:—

a. With the solid Ring.

Copper obtained	=11·435 grammes.
Time of electrolysis	=60 minutes.
Mean deflection	=47°·3.
Position of ring P	= 2.

* Another advantage undoubtedly is that the galvanometer requires no variable shunt, by which errors may very easily be introduced.

† *Rep. f. exp. Phys.* xviii. p. 633 (1882).

‡ See latest instructions for use of their torsion galvanometers.

§ Proc. Phys. Soc. v. p. 80 (1882).

Hence the current corresponding to the unit deflection of 45° with the ring vertical =4·531 ampères.

b. With the fine-wire Coil.

Number of Daniells	= 4.
E.M.F. thereof	= 4 × 1·109.
Deflection obtained	=44°·6.
Position of coil P	= 1.

Hence the E.M.F. corresponding to the deflection of 45° with the ring vertical =**4·525** *volts*, the correction for the resistance of the cells being applied.

These two results agree very closely indeed, showing only a difference of 0·13 per cent. This is the more remarkable as the two kinds of measurements have nothing in common, being in fact based upon data quite independent of each other, thus proving that it is admissible to substitute for such instruments the calibration in volts for that in ampères.

A further series given in Tables III. and IV., and carried out with great care, clearly show that, for any given current-strength or E.M.F., the result of the measurement is almost identical in whatever region the readings are taken.

Table III. is obtained with currents from my constant battery passed through the solid ring. The six different current-strengths were obtained by the insertion of suitable resistances, and were as nearly as possible in the proportion of the whole figures 1 to 6.

Table IV. contains measurements with the fine-wire coil, thereby using the E.M.F. of ordinary Bunsen cells connected in series, and varying in number from 2 to 12.

From these Tables it will be seen that, on the one hand, the deflections extend over the greater part of the tangent-scale, *i. e.* from 3°·4 till 78°·2, and, on the other, the position of the coil varies from the multiplying power 1 to 10—the quantities measurable being therefore in the proportion of 1 to 500, yet the accuracy arrived at may be pronounced fully satisfactory. Combining the results of all these measurements with the solid ring and with the wire coil, the mean error of a single reading becomes 0·35 per cent., and the probable error 0·24 per cent.

Table III.—Measurements with the Solid Ring.

Multiplying powers corresponding to the position of the ring.	Current-strength.																	
	C_1.			C_2.			C_3.			C_4.			C_5.			C_6.		
	a.	tan a.	P × tan a	a.	tan a.	P × tan a	a.	tan a.	P × tan a	a.	tan a.	P × tan a	a.	tan a.	P × tan a	a.	tan a.	P × tan a
P=1	30·7	·5938	·594	49·95	1·190	1·19	·65	1·778	1·78	66·8	2·333	2·33	71·35	2·963	2·96	74·7	3·655	3·66
2	16·6	·2981	·596	30·7	·5938	1·19	41·6	·8878	1·78	49·5	1·171	2·34	56·15	1·491	2·98	61·25	1·823	3·65
3	11·2	·1980	·594	21·65	·3969	1·19	30·6	·5914	1·77	37·9	·7785	2·34	44·75	·9913	2·97	50·55	1·215	3·65
4	8·4	·1477	·591	16·65	·2991	1·20	23·85	·4421	1·77	30·4	·5867	2·35	36·7	·7454	2·98	42·5	·9163	3·67
5	6·75	·1183	·592	13·4	·2382	1·19	19·5	·3541	1·77	25·1	·4684	2·34	30·8	·5961	2·98	36·1	·7292	3·65
6	5·7	·0998	·599	11·15	·1971	1·18	16·4	·2943	1·77	21·3	·3899	2·34	26·35	·4953	2·97	31·35	·6092	3·66
7	4·8	·0840	·588	9·65	·1700	1·19	14·15	·2521	1·77	18·4	·3327	2·33	23·0	·4245	2·97	27·5	·5206	3·64
8	4·25	·0743	·594	8·4	·1477	1·18	12·5	·2217	1·77	16·3	·2924	2·34	20·35	·3709	2·97	24·5	·4557	3·65
9	3·8	·0664	·598	7·5	·1317	1·19	11·05	·1953	1·76	14·5	·2586	2·33	18·3	·3307	2·98	21·95	·4030	3·63
10	3·35	·0585	·585	6·7	·1175	1·18	10·0	·1763	1·76	13·1	·2327	2·33	16·5	·2962	2·96	20·0	·3640	3·64
Mean of P × tan a	...	...	·593	...	...	1·187	...	...	1·769	...	...	2·336	...	...	2·973	...	...	3·647
Mean error of obs.	±0·0043 = ·72 p. c.			±0·0060 = ·51 p. c.			±0·0064 = ·36 p. c.			±0·0069 = ·29 p. c.			±0·0074 = ·25 p. c.			±0·0101 = ·28 p. c.		
Prob. error of obs.	±0·0029 = ·49 „			±0·0040 = ·34 „			±0·0043 = ·24 „			±0·0047 = ·20 „			±0·0050 = ·17 „			±0·0068 = ·19 „		
Mean cur. in amp. (P × tan a × c*)	2·69			5·39			8·03			10·61			13·50			16·56		
Ratio	1			2			3			3·9			5			6·1		

* The galvanometer constant c = 4·54 amperes.

TABLE IV.—Measurements with the fine-wire Coil.

Multiplying powers corresponding to the position of the coil.	Electromotive force.																	
	E_1.			E_2.			E_3.			E_4.			E_5.			E_6.		
	α.	tan α.	P×tan α	α.	tan α.	P×tan α	α.	tan α.	P×tan α	α.	tan α.	P×tan α	α.	tan α.	P×tan α	α.	tan α.	P×tan.
P=1	39·2	·8156	·816	58·2	1·613	1·61	67·4	2·402	2·40	72·7	3·211	3·21	76·0	4·011	4·01	78·2	4·787	4·79
2	22·2	·4081	·816	38·55	·8084	1·62	50·3	1·205	2·41	58·05	1·603	3·21	63·5	2·006	4·01	67·4	2·402	4·80
3	15·25	·2727	·818	28·3	·5384	1·62	38·85	·8055	2·42	46·9	1·069	3·21	53·2	1·337	4·01	58·1	1·607	4·82
4	11·55	·2044	·818	22·0	·4040	1·62	31·1	·6032	2·41	38·7	·8012	3·21	45·15	1·005	4·03	50·2	1·200	4·80
5	9·3	·1638	·819	18·0	·3249	1·63	25·8	·4834	2·42	32·7	·6420	3·21	38·7	·8012	4·01	43·8	·9590	4·80
6	7·8	·1370	·822	15·1	·2698	1·62	21·9	·4020	2·41	28·0	·5317	3·19	33·65	·6657	3·99	38·6	·7983	4·79
7	6·7	·1175	·823	13·0	·2309	1·62	19·0	·3443	2·41	24·5	·4557	3·19	29·75	·5716	4·01	34·45	·6860	4·80
8	5·8	·1016	·813	11·35	·2007	1·61	16·8	·3019	2·42	21·8	·4000	3·20	26·5	·4986	3·99	30·9	·5985	4·79
9	5·2	·0910	·819	10·1	·1781	1·60	15·0	·2679	2·41	19·55	·3551	3·20	24·0	·4452	4·01	28·15	·5351	4·82
10	4·7	·0822	·822	9·15	·1611	1·61	13·55	·2410	2·41	17·7	·3191	3·19	21·7	·3979	3·98	25·55	·4781	4·78
Mean of P×tan α	...	...	·819	...	...	1·615	...	...	2·413	...	...	3·201	...	...	4·003	...	...	4·798
Mean error of obs.	±0·0032=·39 p. c.			±0·0064=·40 p. c.			±0·0044=·18 p. c.			±0·0083=·26 p. c.			±0·0119=·30 p. c.			±0·0129=·27 p. c.		
Prob. error of obs.	±0·0021=·26 ,,			±0·0043=·27 ,,			±0·0029=·12 ,,			±0·0056=·17 ,,			±0·0084=·21 ,,			±0·0087=·18 ,,		
Mean E.M.F. in volts. (P×tan α ×e*.)	3·72			7·34			10·95			14·53			18·17			21·78		
Ratio	1			2			2·9			4			5			6		

* The galvanometer constant e =4·54 volts.

TABLE V.

Position of compensating magnet.	Extra resistance inserted.	Sine of inclination.						Mean of $\frac{\tan\alpha}{\sin\phi}$.	Mean error of observation.
		$\sin\phi$	·2	·4	·6	·8	1·0		
Dipped to augment earth's magnetism.	0	deflect. α	22°·1	38°·9	50°·4	58°·2	63°·5	2·020	$\pm$0·005 or 0·25 p. c.
		$\tan\alpha$	·4061	·8069	1·209	1·613	2·006		
		$\frac{\tan\alpha}{\sin\phi}$	2·03	2·02	2·02	2·02	2·01		
Vertical, having no action.	0·13 ohm.	deflect. α	21°·9	38°·7	50°·3	58°·0	63°·5	2·006	$\pm$0·004 or 0·20 p. c.
		$\tan\alpha$	·4020	·8012	1·205	1·600	2·006		
		$\frac{\tan\alpha}{\sin\phi}$	2·01	2·00	2·01	2·00	2·01		
Dipped to reduce earth's magnetism.	0·56 ohm.	deflect. α	21°·9	38°·9	50°·4	58°·2	63°·5	2·016	$\pm$0·004 or 0·20 p. c.
		$\tan\alpha$	·4020	·8069	1·209	1·613	2·006		
		$\frac{\tan\alpha}{\sin\phi}$	2·01	2·02	2·02	2·02	2·01		

The last set of measurements had for its object to show that a compensating-magnet of the description proposed does not affect the readings. The results are embodied in Table V.; they were obtained with one of the older forms of solid-ring galvanometers provided with a sine-scale. The curved controlling magnet of a mirror-galvanometer, 20 centim. long and 2 centim. broad, was strongly magnetized and placed a a distance of 24 centim. in the manner formerly specified, and so arranged that it could be turned on a horizontal axis. Three different positions were given to the magnet—viz. one, in which it assisted the earth's magnetism, another, in which it did not act upon the needle, and a third, in which the earth's magnetism was partly neutralized. By altering the resistance in circuit, the deflections with the vertical ring were made equal in all three cases, viz. 63°·5.

Table V. shows that the degree of accuracy did not materially differ under the three varying conditions. The magnet therefore does not appreciably interfere with the measurements. The mean error of all three positions of the magnet is 0·22 per cent., which is very low.

In conclusion, I may mention that a smaller model of the galvanometer, intended for practical use, is now being made, which will contain all the recent improvements, viz. the fine-wire coil besides the solid ring, the tangent-scale, the secant-marks, the air-damping, and the compensating-magnet. The latter will be so arranged that the "constant" will be considerably increased as compared with that due to the earth's magnetism alone; thus the needle should be much less influenced by outer disturbances than before.

Woolwich, June 1883.

XXXIV. *Note on the Measurement of the Electric Resistance of Liquids. By* Professors W. E. Ayrton, *F.R.S., and* John Perry, *M.E.**

[Plate XIII.]

Some time back a paper was communicated by Professors Reinold and Rücker to this Society on the Resistance of Liquid Films, which had a double interest, arising from the great

* Read June 9, 1883.

value of the results arrived at and from the method employed to obtain them. It is of course well known that the great difficulty in measuring the resistance of a liquid arises from the polarization of, or actual deposit of gases on, the anode and cathode, which makes the apparent resistance of the liquid far greater than the true value. To overcome this difficulty Kohlrausch employed rapidly alternating currents; and Dr. Guthrie, with Mr. Boys, dispensed altogether with the anode and cathode by observing the amount of twist produced in a fine steel wire supporting a vessel of liquid when a magnet was rotated at a fixed speed in the neighbourhood.

But there is another method of measuring the resistance of a liquid independently of its polarization—the one so successfully employed by Profs. Reinold and Rücker, and which consists in measuring by means of an electrometer the potential-difference at two fixed points in a column of the liquid when a current of known strength is passing through it.

At the time Profs. Reinold and Rücker communicated their paper, we mentioned that some years previously certain experiments had been conducted in our laboratory in Japan for the purpose of ascertaining how far the electrometer method of measuring the resistance of a liquid was entirely independent of polarization; and as we have since come across the results of these experiments in turning over some papers, we have thought that the information may possess some interest for the Members of this Society. The experiments were made at the commencement of 1878 by some of our students; and the first part of the investigation was for the purpose of ascertaining how the resistance of water varied with the electromotive force employed and with the temperature of the water when, first, the resistance was measured by the current which a known electromotive force could send between platinum plates of known size and at fixed distances apart in the water, and, secondly, when the resistance was measured by a comparison of the potential-differences of two platinum wires placed in the water at fixed distances apart, with the potential-differences when the same current was being sent through a known resistance.

Figs. 1 and 2, Pl. XIII., show the arrangement of the apparatus used in the experiments. B is the battery producing the

current passing between the platinum plates P and P′. G is a delicate reflecting galvanometer measuring the current. E is a quadrant-electrometer which measures the difference of potentials between the two wires W and W′. These two platinum wires W and W′ were immersed in glass tubes; and their ends were above the bottom of the glass tubes as shown. Figure 1 shows the connexions when the differences of potentials between W and W′ were being measured by the electrometer, and figure 2 when the differences of potentials at the two ends of the known resistance-coil, of 10,000 ohms, were being measured.

The following Table gives the dimensions of the various parts of the apparatus:—

Diameter of the beaker at water-line	8·5	centim.
Height of water-line above the bottom ...	5·76	"
Distance between centres of wire tubes (W, W′ in fig. 2)........................	4·88	"
Distance between the platinum plates......	7·3	
Part of the glass tube surrounding the wire dipped in water......................	2·14	"
Part of the platinum wire in water.........	0·91	"
Outside diameter of the glass tube	0·87	"
Size of platinum plate : height	3·28	"
" " " : width	2·29	"

Before each experiment, when no current was passing, the difference of potentials between the plates and wires was reduced to 0, if not 0 already. The wires W and W′ were heated to redness before each experiment, and the platinum plates cleaned.

At the beginning pure distilled water was used; and this water was not added to all the time: it therefore lost a little by evaporation during the course of the experiment, and may have become a little dusty; but as the main object of the investigation was to examine the method of testing, and not for the purpose of measuring the specific resistance of water or of any other particular liquid, this result was of little consequence.

The following is a sample of the experiments made:—

January 25, 1878.—Battery-power employed $\frac{1}{6}$ of 23 Daniell's cells, having an E.M.F. of 4·08 volts, and which gave a deflection of 468 divisions on the galvanometer when

shunted with the $\frac{1}{749}$ shunt, and when a resistance of 10,000 ohms was introduced in the circuit.

Time after putting on battery.	Galvanometer-deflection.	Electrometer-deflection.		Temperature.
		Figure 1.	Figure 2.	
1^m	99	10		59°·5 F.
2	96	10		
3	94	10		
Plates and wires thoroughly discharged.				
1^m	99	......	14	61° F.
2	98	......	14	
3	90	......	14	

Time after putting on battery.	Resistance as determined by the galvanometer.	Resistance as determined by the electrometer.
1^m	37000	7100
2	38000	7100
3	39000	7100

The annexed Table gives the results of a long series of experiments:—

Total electromotive force, in volts.	Temperature.	Resistance as determined by galvanometer at end of one minute.	Ratio of resistance at the end of second minute to the resistance at the end of the first.	Ratio of resistance at the end of third minute to the resistance at the end of the first.	Resistance as determined by electrometer.	Ratio of resistance determined by galvanometer to resistance as determined by electrometer.	Date.
0·93	58° F.	93900	1·27	1·4	15000	6·26	23rd Jan.
	60	133000	1·12	1·37	15000	8·87	23rd "
A 0·93	100	56000	1·16	1·27	8000	7·00	24th "
1·86	63	53000	1·10	1·14	10670	5	24th "
1·86	62	51500	1·12	1·18	10300	5	24th "
B 1·86	102	32000	1·17	1·27	9670	3·31	24th "
4·08	59·5	37000	1·03	1·05	7100	5·21	25th "
4·08	102	19800	1·01	1·06	3270	6·06	28th "
6·17	60	21000	1·0	1·0	4450	4·72	28th "
C 6·17	106	12000	1·08	1·08	2700	4·44	31st "
16·45	62	12700	1·01	1·99	3170	4·01	31st "
D 16·45	107	7700	1·04	1·04	1953	3·94	31st "

Only one number is given for the resistance as determined by the electrometer in each case, because it was found not to vary much during the time of electrification; whereas the resistance as determined by the galvanometer, as will be seen, increased in the earlier experiments 30 to 40 per cent. during the three minutes' electrification.

The total electromotive force in each case was determined by making a comparison by means of the electrometer with one of Clark's standard cells.

From these observations the following conclusions may be drawn:—First, the resistance measured by the galvanometer is much greater when using about 1 volt than when using nearly 2, at the same temperature (compare observations A and B), whereas the electrometer-measurements altered very little at all. Again, comparing C and D, we see that the resistance measured by the galvanometer is much greater when using 6 volts than when using 16. In this case, however, the measurements of the electrometer are also considerably greater in the first case than in the second, the temperature being the same. Secondly, if the electromotive force is less than the decomposing electromotive force, then the smaller it is the more does the resistance alter from one to two minutes' electrification, and from two to three minutes'. Whenever, however, the electromotive force is sufficiently high for decomposition to take place, the electrification seems to produce but little change in the resistance. The resistance of the water diminishes as the temperature rises, the electromotive force being kept constant

The following experiments were made preliminarily to explorations of the region between the two platinum plates in the water, for determining what were the directions of the lines of flow of current. We desired to see if there was any chance of being able to use platinum wires in glass tubes connected with the electrometer, as previously described.

In the following cases a long trough of water was used instead of the beaker.

The sensibility of the galvanometer was nearly the same throughout all the experiments, and was such that $\frac{1}{20}$ of the whole electromotive force employed produced a deflection of about 500 divisions when there was an external resistance

of 10,000 ohms and when the multiplying-power of the shunt employed was 100·7, which shunt was used throughout all the experiments.

Four Menotti cells, having an electromotive force of 3·7 volts, were employed in each of the following experiments. In A, B, C, D, E, F, and G the two platinum plates were placed parallel to one another at a distance of 90 centimetres apart. The two wires and their glass tubes were placed to commence at a distance of 80 centim.—that is, each being 5 centim. from the platinum plate. The lower ends of the

February 21, 1878.

	Distance between platinum wires.	Time after putting on battery.	Galvanometer-deflection.	Electrometer-deflection.	Temperature of water.
	centim.	m s			
A	80	1 0	669	53	13° C.
	60	1 30	662	40	
	40	1 50	658	27	
	20	2 15	654	15	
B	80	1 0	667	52	13° C.
	60	1 25	663	39	
	40	1 45	657	27	
	20	2 10	653	15	

February 22, 1878.

	Distance between platinum wires.	Time after putting on battery.	Galvanometer-deflection.	Electrometer-deflection.	Temperature of water.
C	80	1 0	680	50	13° C.
	60	1 35	675	37	
	40	2 10	672	26	
	20	2 30	670	15	
D	80	1 0	685	49	13° C.
	60	1 20	680	37	
	40	1 50	677	24	
	20	2 10	674	12	
E	80	1 0	697	48	13° C.
	60	1 20	692	36	
	40	1 50	688	24	
	20	2 20	685	13	
F	80	1 0	699	50	13° C.
	60	1 35	694	37	
	40	1 50	690	25	
	20	2 15	687	13	
G	80	1 0	over 717	51	13° C.
	60	0 30	„	38	
	40	2 0	„	25	
	20	2 20	„	15	

platinum wires were each $1\frac{1}{2}$ centim. above the lower ends of the glass tubes; and the lower ends of the glass tubes were 1 centim. below the surface of the water. The two platinum plates and one of the platinum wires were kept immovable, while the other platinum wire was moved along the trough.

The object, of course, of taking the galvanometer-readings was to ensure that no material change was taking place in the current through the weakening of the battery or otherwise while the experiment was being made.

The experiments E and F appear most satisfactory of this set; and from these it seems that the resistance of the upper layer of water-column is nearly proportional to the distance between the platinum wires, except for the nearest distance, n which case the column seems to have a slightly larger resistance than it ought to have. This perhaps arose from the fact that, although the platinum plates nearly filled up the entire section of the trough, still the lines of flow at the platinum wire, which was kept stationary at a distance of 5 centimetres from one of the plates, were not quite parallel to the edge of the trough.

The two following sets of experiments, H and I, differ from the preceding only in that the lower end of the glass tube was one centimetre above the bottom of the trough; and from these two sets of experiments we see that the resistance of the lower layer of water-column, as measured by the electrometer, is nearly proportional to the distance between the wires, except, again, for the shortest distance.

	Distance between platinum wires.	Time after putting on battery.	Galvanometer-deflection.	Electrometer-deflection.
		m s		
H	80	1 0	709	50
	60	1 35	703	37·5
	40	2 0	700	26
	20	2 20	698	13
I	80	1 0	704	49
	60	1 20	700	37
	40	1 45	796	25
	20	2 20	792	13

The next experiments were for the purpose of seeing whether

the potential, as measured by the electrometer, would come out uniform at all points in one vertical transverse section of the trough as well as at all points in one of the glass tubes.

	Distance between the platinum wires W and W′, in centimetres.	Position of the lower end of one of the tubes.	Galvanometer-deflection.	Electrometer-deflection.
J	80	Up.	696	49
	80	Down.	692	49
	80	Up.	689	49

"Up" means that the lower end of the glass tube was about 1 centim. below the surface of the water; and "Down" that it was about 1 centim. above the bottom of the glass trough. The platinum wire was now raised about 4 centim. above the bottom of the glass tube when the glass tube was down and the electromotive force was unaltered. The potential therefore, at all points in a vertical transverse section as well as at all points in the glass tube, is the same as measured by the electrometer.

The next set of experiments, K and L, were made under exactly the same conditions as A, B, C, D, E, F, and G, with the exception that the terminal platinum plates were now perpendicular to each other, the plate towards which the wire was moved being parallel to the long side of the trough.

	Distance between the platinum wires, in centimetres.	Time after putting on battery.	Galvanometer-deflection.	Electrometer-deflection.	Temperature.
		m s			
K	80	1 0	706	51	13° C.
	60	0 30	701	38	
	40	0 50	699	27	
	20	2 0	697	15	
L	80	1 0	705	51	13° C.
	60	0 30	701	40	
	40	0 55	698	27	
	20	2 20	697	15	

The resistance of the longer column of the water as measured by the electrometer is about the same as before, whereas that of the shorter is even greater; so that the resistance for the 80-centimetre column is even still less than four times that for the 20-centimetre one. But since the platinum plate near the stationary platinum wire was in these last two sets of experiments K and L kept parallel to the trough (that is, parallel to the mean direction of the lines of flow), it follows that any want of parallelism of the lines of flow to the edge of the trough at the point where was the stationary wire would be exaggerated by this mode of placing the plate; and since we observe that the error in the proportional law for distance is also increased, we may conclude that the explanation given above of the want of perfect accuracy in the proportional law being due to want of perfect parallelism in the lines of flow is the correct one.

In all the previous experiments the distance between the electrometer-wires only was altered; but in the next set the distance between the platinum plates as well as that between the platinum wires was altered, the distance between each plate and wire being kept constant. Further, the resistance determined from the electrometer was calculated, not, as before, by comparing the electrometer-deflection when its electrodes were attached to the platinum wires with the deflection obtained when its ends were attached to a known resistance traversed by the same current, but by first determining the absolute value, in volts, of the electrometer-scale with the absolute value, in ampères, of the galvanometer-scale, and by observing the electrometer- and galvanometer-deflections in each experiment.

Battery-power employed, 4 Menotti's cells. Temp. 14° C.

Time after putting on battery, in minutes.	Galvanometer-deflection.	Electrometer-deflection.	Resistance, as determined by galvanometer, in ohms.	Resistance, as determined by electrometer, in ohms.
Distance between the platinum plates 20 centim. „ „ „ wires 10 „			Shunt $\frac{1}{990 \cdot 2}$.	
1	230	19	27000	11340
2	220	19	28000	11760
3	214	18·5	29000	11900
4	210	18	29600	11800
5	206	17	30000	11400
Distance between the platinum plates 90 centim. „ „ „ wires 80 „			Shunt $\frac{1}{100 \cdot 7}$.	
1	634	42	97000	90200
2	621	42	98980	92070
3	614	42	99960	92900
4	608	41	100940	91900
5	602	41	111960	101900
Distance between the platinum plates 90 centim. „ „ „ wires 80 „			Shunt $\frac{1}{100 \cdot 7}$.	
1	633	43	97000	93100
3	621	42	98000	91100
4	614	42	99000	92100
5	609	42	100000	93000
6	604	41	101000	92000
Distance between the platinum plates 20 centim. „ „ „ wires 10 „			Shunt $\frac{1}{990 \cdot 2}$.	
1	245	21	25500	11900
2	235	20	26600	11700
3	230	20	27200	11960
4	225	19	27800	11670
5	220	19	28400	11930

The resistance, therefore, as measured by the galvanometer,

does not increase as rapidly as the distance separating the plates, while that as measured by the electrometer is fairly in proportion to the distance. The explanation of the former is probably due to the fact that, since the electromotive force employed in all these four sets of experiments was constant, a greater current flowed when the plates were nearer than when they were far apart, hence that the resistance due to the layer of gas was greater when the plates were near than when they were far.

And this leads to a simple method of accurately measuring the resistance of liquids by using a galvanometer. The method, which was independently arrived at by one of our assistants (Mr. Mather), is now employed in our laboratory, and is so simple that we feel it can hardly be novel. It is as follows :—In a long vertical glass tube containing the liquid there are two metallic disks, not necessarily of platinum, and of about the same diameter as the tube. One of these can slide up and down the tube, so as to be able to be set at any fixed distance from the other. The disks are first put tolerably far apart, and a certain convenient current made to flow, which is measured on a galvanometer in the circuit. The plates are now made to approach and the current kept exactly the same by the insertion of an external resistance; whence it follows that the resistance of the column of liquid which has been subtracted from that originally separating the plates is equal exactly to the external resistance necessary to be inserted to keep the current constant.

February 28, 1878.

The next set of experiments was made to determine the alteration in resistance of a long trough of water when the distance between the centres of the platinum plates was kept constant at 90 centimetres, and the positions of the platinum plates varied as shown in the figures.

Galvanometer-Constant.—4 Menotti's cells with an E.M.F. 3·8 volts gave a deflection of 618 when a resistance of 10,000 ohms was in circuit and the galvanometer shunted with the $\frac{1}{990\cdot 2}$ shunt.

The 4 Menotti's cells were employed and the galvanometer shunted with the $\frac{1}{100\cdot 7}$ shunt, and the readings were

in each case taken one minute after the application of the battery.

<table>
<tr><th></th><th>Position of plates.</th><th>Galvanometer-deflection.</th><th>Temperature.</th></tr>
<tr><td rowspan="3">I.</td><td>— —</td><td>642</td><td rowspan="3">16° C.</td></tr>
<tr><td>\ \</td><td>640</td></tr>
<tr><td>| |</td><td>622</td></tr>
<tr><td rowspan="3">II.</td><td>| |</td><td>634</td><td rowspan="3">16° C.</td></tr>
<tr><td>\ \</td><td>643</td></tr>
<tr><td>— —</td><td>647</td></tr>
</table>

Two sets of experiments in the reverse order were taken to eliminate any change that might take place in the deflection from weakening of the battery, or from polarization of the plates, or from set of the galvanometer-fibre. The constant distance between the centres of the plates was now diminished to 20 centimetres, when the following results were obtained, the $\frac{1}{990\cdot2}$ shunt being employed.

<table>
<tr><th></th><th>Position of plates.</th><th>Galvanometer-deflection.</th><th>Temperature.</th></tr>
<tr><td rowspan="3">III.</td><td>| |</td><td>245</td><td rowspan="3">16° C.</td></tr>
<tr><td>\ \</td><td>291</td></tr>
<tr><td>— —</td><td>304</td></tr>
<tr><td rowspan="3">IV.</td><td>— —</td><td>315</td><td rowspan="3">16° C.</td></tr>
<tr><td>\ \</td><td>285</td></tr>
<tr><td>| |</td><td>239</td></tr>
</table>

Both therefore at the greater and at the less distance the resistance is least when the platinum plates are edge on ; a result that could hardly have been expected for the longer distance, considering that the width of each plate was only about 6 centimetres.

March 1, 1878.

In the following experiments one plate only was turned. The galvanometer had about the same sensibility as before. The $\frac{1}{100\cdot7}$ shunt was used when the distance between the

centre of the plates was 90 centimetres, and the $\frac{1}{990\cdot 2}$ shunt when it was 20 centimetres. An electromotive force of 3·8 volts was employed in each test.

Distance between the centres of the plates 90 centimetres.

	Position of plates.	Galvanometer-deflection.	Temperature.
V.	| |	633	15° C.
	| \	634	
	| —	637	
VI.	| —	660	15° C.
	| \	655	
	| |	651	

Distance between the centres of the plates 20 centimetres.

	Position of plates.	Galvanometer-deflection.	Temperature.
VII.	| —	403	15° C.
	| \	368	
	| |	358	
	Battery reversed.		
VIII.	| |	208	
	| \	211	
	| |	229	

Here again, then, the resistance is least with the plate end on, even when the distance between the centres of the plates is as much as 80 centimetres.

This apparent anomaly of the smaller resistance obtained when one or both plates is put end on is, as was pointed out by Mr. Boys, probably due to the smaller density of the gas which is deposited on a plate when it is put end on (in consequence of the current flowing from both sides of the plate into the liquid under these circumstances) more than compensating for the want of parallelism of the lines of flow when one or both of the plates are put end on.